LARRY LAUDAN'S THEORY OF SCIENTIFIC PROGRESS

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By

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CERTIFICATE

This is to certify that the work presented in the Dissertation titled ***"Larry Laudan's Theory of Scientific Progress"*** is carried out by ***Saima Hameed*** under my supervision and can be submitted in partial fulfilment of the requirements for the award of the degree of Masters in Philosophy.

I am satisfied with the effort and hard work put in the completion of this dissertation.

(Mohammad Muqim)
Associate professor
(Supervisor)

Dedicated
to
My Family
&
Nida

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"Allah alone is besought for help and on Him alone we depend".

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Saima Hameed

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CHAPTER – I

Introduction

INTRODUCTION

Philosophy of science can be said to be as old as philosophy itself. Science has been regarded as a paradigm of privileged knowledge since ancient Greek civilisations. Science has been deemed to be systematic and reliable knowledge of the physical entities. It has been compared to mere opinions or ungrounded beliefs. Greek philosophers were the first to clearly and categorically take science as the most probable knowledge.

In the west, the history of the philosophy of science begins with the Greek philosophers, especially with refrence to Plato and Aristotle. For Plato, man was blessed with an innate knowledge of everything. Learning was merely a process of unlocking of our innate understanding of phenomenal and non-phenomenal realities and that knowledge was essentially eternal and universal and acquired through innate rational intuition. Empirical knowledge gained through sense-experience was considered opinion. The real knowledge could be advanced by recourse to deduction alone. Aristotle, on the other, charged Plato of utopianism and forwarding of ideals and models which did not really exist. As against Plato, Aristotle held that real and pure scientific knowledge could be achieved only by observational and inductive reasoning. Aristotle underlined that gathering of knowledge was a process of gaining experience. It was building upon what is already known to be true. In view of the same, each scientific discipline needed to be built upon the first principles. Thus Aristotle was the first thinker to clearly formulate scientific method which subsequently was re-appropriated by modern philosophers as well as practitioners of science leading to scientific achievements and breakthroughs in our times. That is why Aristotle as the initiator of scientific method is also credited as the father of science. Aristotle deemed science as a stable and deductive structure based on first principles. These first principles are arrived at by a process that Aristotle called 'induction'. For Aristotelians science is a deductive axiomatic structure of knowledge. It aims at causal explanations based on first principles with regard to essence of things.

There is no and possibly there cannot be any complete or perfect definition of the philosophy of science. However, by way of illustration, we can bring out various dimensions or features of philosophy of science. The philosophy of science tries to understand the methods, products or results and aims and objectives of science. It attempts to furnish us a comprehensive understanding of the procedures, techniques and canons of scientific research and investigation.

Philosophy of science is a branch of philosophy that is centred on a critical examination of science; its methods and results. Methodology as a branch of philosophy of science is closely related to the theory of knowledge. It explores the methods by which science arrives at its truths concerning the world. It critically explores various rationales for these methods. It explores the criteria of the acceptability of scientific theories. It also examines the relation between evidence and hypotheses. It also examines the degree to which scientific claims can be falsified by observational data. Other problems examined by the philosophy of science are the nature of scientific laws, the cognitive content of scientific theories referring to unobservable and the structure of scientific explanations. It also explores specific foundational questions arising out of the specific results of the sciences. For example, it might explore the metaphysical presuppositions of theories about space and time, the role of probability in statistical physics, the interpretation of measurement in quantum theory, the structure of explanation in evolutionary biology and so on.

Historically speaking, philosophers of science have directed their efforts towards the discovery of pattern, system or structure in science. They have especially laid emphasis on discovery of order in scientific investigations. The universe we encounter is full of anomalies and antinomies. Finding a rational or systematic order across this vast spectrum is desired by almost all human beings. However, those who commit themselves to a systematic exploration and explanation of order in the universe are designated as scientists. The main occupation of a scientist is problem-solving with the goal of understanding the universe. Such an undertaking requires explanations. The quest for explanation is the characterising feature of scientific enterprise. Understanding of the requirement of an explanation is to grasp a pervasive feature of science. Seeking explanations is a basic feature of science. The fundamental task of science is to organise and systematize our knowledge of what goes on in the world on the basis of explanatory principles.

Philosophers of science have been centrally concerned to work out and clarify the modes and methods of doing science or exploring cannons of rationality leading to true and objective knowledge. Philosophers such as Bacon, Descartes, Locke, Kant, Mill, and other philosophers of science up to twentieth century logical positivists, all were trying to explore cannons of scientific verification, confirmation and justification. They were concerned to explore those cannons of rationality which if followed can lead to true and objective scientific knowledge or which at least can lead to an understanding of the limits of such knowledge.

On the simplest level, science is knowledge of the natural world. This knowledge is constituted by recognising the regularities in nature. For example, recognition of regularity in which, the sun and the moon periodically repeat their movements, is scientific knowledge. However, the mere recognition of regularities does not exhaust the full meaning of science. Regularities can be construction of the human mind. Human mind normally likes to explore regularities and uniformities and does not like to see chaos in nature; so it may construct regularities even when none objectively exists.

Some philosophers of science insist that understanding of regularities demands explanations of scientific causes and laws. However, such a stance is highly debatable according to many contemporary philosophers of science. While modern biology thrives on causal chains, modern quantum mechanics has given up the causal quest. Even if causation and explanation are admitted as necessary, there is little agreement on the kind of causes that are permissible or possible in science. Now if the causes that were acceptable to previous scientists are unacceptable to contemporary ones, it is reasonable to assume that causes accepted by contemporary scientists will not be acceptable to scientists of the future. Therefore, the contention that science is to be considered as knowledge of regularities needs to be subjected to sceptical reasoning.

The typical scientific mindset suspends beliefs in a position until appropriate evidence is obtained with regard to the truth of the proposition under consideration. A scientist believes in the truth of the proposition only to the degree that the available evidence warrants. However, the philosopher of science will have to point out that the nature of evidence depends upon the nature of the proposition. For example, a theory about the origin of the stars cannot be experimentally verified the way we can verify the molecular structure of the water. The law of universal gravitation cannot be verified the way laws of algebra can be demonstrated. One of the crucial functions of the philosophy of science is to distinguish different standards of justification of beliefs. The philosophers of science have to distinguish different kinds of proofs corresponding to different kinds of propositions.

Science is like a dialogue between man and nature. It is the candle in the dark, so-to say, illuminating or clarifying our irrational beliefs or superstitions. Science may not guide us in courses of human action, but it can certainly illuminate the possible consequences of alternative courses. Science is any system of knowledge that is concerned with the physical world and entails unbiased observations and systematic experimentations. In general, science involves pursuit of knowledge covering general truths or the operations of the fundamental laws of nature.

Philosophy and science were indistinguishable up to late eighteenth and early nineteenth centuries. Scientists were known as natural philosophers. In fact, ancient and medieval thinkers were, philosophers, theologians, scientists and historians all rolled into one. In the history of human thought scientific ontologies and methodologies grew simultaneously along with philosophical interpretations and theological world-views and value systems.

Contemporary philosophers of science do not underline that science is entirely directed or dictated by rational methods and objective cannons. They can accept that a historical survey of science can reveal that science has to a considerable extent been methodologically directed or rationally supervised in its exploration of physical laws and logico-mathematical deductions. However, the history of science also reveals that science cannot be satisfied to be a series of methodologically directed programmes. Some of the most spectacular achievements of science have not been fully explainable in terms of the prescriptions of 'inductivist', 'deductivist', 'verificationist' or 'falsificationist' methodologies. In particular, there is mounting historical evidence that much of scientific reasoning has not been inductive especially in the empiricist or positivist sense.

Philosophers of science since Aristotle via Bacon, Mill, Whewell, Duhem, Poincare and Mach have been trying to figure out the nature, method, function and scope of science. In twentieth century, as against philosophical accounts of science, historical investigations with regard to the development of science have become widely popular. Most of the people who were interested in philosophical issues and historical concerns of science opine that history of science and philosophy of science are radically different. The historian of science is dealing with facts and data. He wants to arrange facts and data about science into a convincing and coherent account with a view to providing us awareness about the evolution of science. By contrast, philosophy of science is largely understood as a normative, evolutional and a priori investigation of how science ought to go on. History of science deals with matters of fact with regard to science. On the other hand, philosophy of science is oriented to matters of value with regard to nature, direction, methodology and goals of science. The historians of science are concerned about how science has developed in course of time whereas the philosophy of science has been concerned about how science should proceed. Philosophy of science is concerned with foundational problems that arise within science and tries to understand science as a cognitive activity that is capable of providing us justified true beliefs about the world. It is centrally concerned with the method as well as goal of science. Determining the nature of science and bringing out its differences with non-science and pseudo-science, is also one of the crucial assignments of philosophy of science. Philosophy of science also tries to explore the nature of scientific theories and their relation to the world. It seeks to explore how theoretical concepts get their meaning and how they are related to observations. It also tries to explore the structure and content of such concepts as causation, explanation, confirmation, experiment, model, reduction, probability etc. It also tries to find out the role of values in scientific decisions and their relationship to social, cultural and historical perspectives.

In Twentieth century, philosophy of science emerged as a clear, categorical and distinct sub-discipline of philosophy. Philosophy of science was pioneered by philosophically informed scientists who negotiated acute theoretical battles over the validity and truth of emergence of scientific theories. They felt the need to understand the aim and structure of scientific theories. They became deeply interested in understanding the role of hypotheses and experiments in science. They struggled to understand the nature of scientific justification and limits of scientific explanation. Philosophers of science and scientists such as Pierre Duhem, Henri Poincre, Ludwig Boltzmann, Henrich Hurtz, Ernest Mach, Max planck, Albert Einstein etc authored articulate methodological and philosophical books with regard to nature of scientific theorising and scientific method. However, three developments around twentieth century can be cited as instrumental in the development of philosophy of science. Firstly, achievements in the foundation of logic and mathematics exemplified by Hilbert's Geometry and Russell's Logic provided stimulus to the fully-fledged emergence of philosophy of science. Secondly, a reaction against metaphysics such as Hegelianism by scientifically and mathematically inclined philosophers such as Russell inspired philosophers of science such as logical positivists to go in for establishing philosophy of science on sounder and sharper methodological foundations. Thirdly, revolutionary developments in the field of physics such as theory of relativity and quantum mechanics, generated philosophical interest in the nature of space, time and causality etc, leading to further stabilisation of the field of philosophy of science.

One of the most critical and crucial jobs or assignments of philosophy of science is to examine the process of arriving at scientific hypotheses or formulating scientific theories. There is no royal road to discovering hypotheses of formulating theories. Such a process entails creative genius and great imagination. Scientific theories are mostly incompatible or disconcerted with facts of bewildering varity. Sometimes scientific theories can have equal capacity for explaining the relevant facts. So, there is no clear-cut method of determining which one of the rival theories is more acceptable. Philosophers of science have evolved various conditions of acceptability. They have argued that a hypothesis which is more significant in-terms of explaining facts must be deemed to be scientifically more fruitful and acceptable. Secondly, if a hypothesis is more testable than its rival hypotheses then it should be deemed to be more acceptable. Thirdly, if a hypothesis is more compatible with previously well-established hypotheses or is consistent with well-confirmed hypotheses then its acceptability is enhanced. Fourthly, a hypothesis that has greater predictive power from which we can deduce greater range of observable facts should be deemed to be more acceptable than its competing hypotheses. Fifthly, even when rival hypotheses can explain all the known data but one is simpler than others that hypothesis should be deemed to be more acceptable. However, there is no necessary connection between such conditions of acceptability of scientific hypotheses and the real worth of an upcoming scientific theory. Philosophers of science are aware of the puzzling features of this acceptability – unacceptability debate. Subsequently, they underline that we should be on guard while estimating the value and worth of rival scientific theories.

All scientific disciplines register successive theories in course of time. The new theories are almost different from old ones. However, the difference between theories or change from old theories to the new theories cannot be said to be signifying scientific progress. A change can be considered a progress only if the new theory is certified to be a better theory or explanation of the problem under consideration than the previous one. Scientific progress signifies success of scientists in arriving at better and better theories. However, scientific progress is an ongoing phenomenon. It is an infinite journey dedicated to a complete explanation or set of explanations with a view to deciphering the mystery of the cosmos. We cannot even tabulate the steps or illustrate the procedures that can lead us to that ultimate goal of scientific research. The progress of science is endless in view of several reasons; firstly, there is always an evergrowing sophistication of scientific techniques, instruments, strategies, and methods of science. The quest for sophistication, precision and exactness is simply limitless. Secondly, even when going by apparent physical, chemical and biological phenomena, the world is infinitely complicated. All things or phenomena have infinite properties. We can discover some of them as and when we succeed in cracking a problem under-consideration. However, there are infinitely complicated factors that intervene, making the scientific struggle all the more, complicated and multi-dimensional. Thirdly, the world is unfathomably profoundly complex. For instance, the structure of matter consists of different levels. These levels are, too complicated, too complex, too multi-dimensional and almost too infinite to once for all yield to us all the secrets. In view of these considerations, scientists should forever go on discovering new fundamental elements of matter along with new basic laws. Scientific revolutions have got to be an ongoing process. Scientific progress is infinite because it is associated with infinite complexity of the world. Scientific research cannot be taken in by the appearances. It has to go to the essence of the phenomena and negotiate several orders of appearances and essences. This process is never-ending and ever-going. At best science can be said to be progressing from theory to theory negotiating a series of better and better deductive systems. We can visualize scientific progress from problem to problem and the solution of higher levels can always be an ongoing process.

Scientific problems arise out of our dissatisfactions. They arise when they do not yield results according to our expectations. They also arise when our theories entangle us in troublesome disagreements. Problems can arise intra-theoretically as well as inter-theoretically. They can arise when our theories negotiate irresolvable conflicts with our observations. However, theoretical constructions or formulations can arise only after negotiating the problem. The crystallisation or articulation of a problem goads us to face challenges, to observe things in new ways and to press on for possible solutions.

Science originates from problems and not from observations. Of course, observations are vital to the rise of problem if they disagree with our theories. The solution of a problem through the construction of a theory is the overriding concern of a scientist. Each significant new theory raises new problems; problems of reconciliations and problems of carrying out unthought-of observational tests. Scientific research is fruitful and rewarding to the extent it leads to novel problems. Suggestion of new problems is the most vital input to the development of scientific knowledge. A theory can make long-lasting contribution to the growth of science by suggesting new problems or troubles in the ongoing process of scientific research. It is through new problems that problems of an ever-rising profundity can be suggested to competent practitioners of science. Scientific research is really a circulatory journey from problems to theoretical constructions and theoretical constructions to problems. Such a circulatory process leads to ever-increasing advancement to more sophisticated results and findings.

Historical examples can be cited with a view to illustrating progress of science. For example, Kepler's and Galileo's theoretical constructions were combined and superseded by Newton as Fresnel's and Faraday's theoretical constructions were combined and superseded by Maxwell. Newton's and Maxwell's theories were logically stronger and better testable. The theories of

Newton and Maxwell were similarly combined and superseded by Einstein's theories. In this ongoing process, scientific progress consisted in achieving more enlightening theories which were more rigorously testable and more simply refutable.

Contemporary philosophers of science have advanced the arguments that scientific progress is not cumulative but discontinuous. Progress is a revolutionary process. Scientific progress consists in accepting radical new ideas and theories and giving up of old ideas. The most important philosopher of science who has underlined the revolutionary view of scientific progress is Thomas Kuhn. Kuhn advanced Copernican astronomy, Darwinian biology and Einsteinian Relativity physics as crucial examples in the history of science justifying revolutionary view of scientific progress.

Scientific progress has axiological or normative connotations. Progress is not a descriptive term like 'change' or 'development'. Progress connotes an improvement with reference to some principles or criteria. Progress also needs to be distinguished from quality. While quality is an activity-oriented concept and signifies the performance of a person with reference to his talent or capability, progress is a result-oriented concept signifying creative breakthrough with reference to some purpose. One can generally hope that high quality research ought to lead to scientific progress. However, there is no necessary relationship between the quality and progress of scientific research. At times, even high quality research may not lead to important results whereas, at times, even low quality researchers can, by some turn of luck, arrive at highly significant results. Scientific discoveries can signify progress when they lead to new ideas which may not necessarily be explainable in logical terms. Scientific progress can be of various types. It can be discovery of observable facts. It can be theoretical descriptions or tests of theories and hypotheses. It can be acceptance or rejection of hypotheses. It can be growth of new logical techniques or application of a universal theory to specific theoretical or practical problems. Scientific progress can be an important discovery, an illuminating analysis, a crucial explanation, a significant theoretical integration, critical development of new ideas and approaches and so on. As it happens, the mysteries of cosmos are inexhaustibly out there, the process of scientific progress has also to be an ongoing perennial affair.

CHAPTER – 2

Perspectives of Scientific Progress

(i) LOGICAL POSITIVISTS: PROGRESS AS VERIFIABILITY

Logical positivism is the philosophy advanced by Vienna circle. Moritz schlick, Rodolf Carnap, Friedrich Wismann, Otto Neurath, Kurt Goldel, Hans Hahn etc. were the leading lights of the circle. Logical positivists of the Vienna Circle were inspired by the positivistic heritage of David Hume and Ernest Mach who had launched radical critiques of metaphysics and underlined the need for empirical investigation. Logical positivists accordingly deemed scientific enquiry as the model for all intellectual investigations. They were convinced that physical sciences were the model for other sciences and all natural and social sciences can be unified and reduced to physical terms. Logical positivists were also powerfully inspired by the logical and mathematical investigations of Frege, Russell and Wittgenstein. The members of the Vienna Circle were categorically convinced that only science delivers knowledge through observational and experimental verification. The knowledge furnished by empirical sciences, is exceptionally privileged and in view of the same methods of science provides the yardstick against which all other claims to knowledge must be measured. Accordingly, the distinctive slogan of logical positivists came to be known as, "the meaning of a statement is the method of its verification" (The Philosophy of Science An Encyclopedia, 2006, p.459)

Logical positivists call themselves logical for they think that their standpoint is necessitated by the logical analysis of language. They divide linguistic discourse into two types – Cognitive and Emotive. It is the cognitive statements which can be said to be descriptions of truth-claims. Logical positivists, furthermore, divide cognitive statements as well into synthetic factual statements and analytic – logicomathematical statements. Only these two types of statements can be cognitively meaningful. Only such statements can be true or false. On the other hand, metaphysical, theological, mystical and ethical statements are non-cognitive or emotive statements and express our attitudes, feelings, emotions etc. only cognitive statements can be said to be meaningful statements, for meaning is the relation between an assertion and a fact or set of facts. Emotive statements are devoid of cognitive significance for they are not assertive of facts but expressions of attitudes or feelings. Emotive statements such as metaphysical propositions can be said to be neither true nor false but meaningless and non-sense (Jamal Khwaja, 1965, p.90).

Logical positivists came to the conclusion that their criterion of meaningfulness, viz; non-analytic statements are cognitively meaningful or significant if they can be verified, makes a sharp and categorical distinction between science and metaphysics (Stathis and Martin, 2008, p. xxii) The single goal of the philosophy of science around Vienna in the 1920's was to bring out somehow that scientific knowledge, in contrast to metaphysics, rests on solid empirical foundations. Logical positivists contended that an analysis of language reveals the cognitive meaningfulness of verifiable scientific propositions only, while emotive propositions are expressions of personal likes and dislikes. Knowledge can only grow by recourse to application of the principle of verification.

Logical positivism like all positivistic responses to the problem of knowledge, has underlined that knowledge is a publically observable, certifiable and acceptable enterprise rather than a privately understandable phenomenon. The mystical, metaphysical, theological and spiritual realms of understanding and interpretation are beyond the pale of public scrutiny. Even the ethical and the aesthetic modes of projection and understanding are impervious to public scrutiny. What is public or common stock of knowledge can be methodically formulated and presented. The knowledge susceptible to method can be repeatedly ascertained by all competent practioners in any given field of investigation (The Philosophy of Science An Encyclopedia. 2006. P.463. Encyclopedia)

Following Hume, Kant and nineteenth century philosophers of science, logical positivists brought out a clear categorical and highly radical account of knowledge in so far as they launched a movement with a view to finally undermine the cognitive claims of such classical disciplines as metaphysics, theology, ethics, aesthetics, mysticism, etc. Their criterion of demarcation between propositions of science and those of metaphysics- theology was the criterion of verification or verifiability. Although such a positivistic criterion of demarcation was, in some sense or the other, available for centuries, logical positivists, against the backdrop of outstanding scientific achievements upto twentieth century, were the most radical group of philosophers who finally achieved a categorical distinction between scientific and non-scientific disciplines. Like Hume who castigated metaphysics to be nothing but sophistry and illusion, logical positivists declared metaphysics to be neither true nor false but meaningless and non-sense. They almost identified knowledge with science. To them knowledge meant scientific knowledge. Non-scientific disciplines of interpretation can be having considerable merits or demerits of their own. But, they can never attain to cognitive meaningfulness, for they can never be subjected to the control of experimental or methodological verification (Jamal Khwaja, 1965, p.76).

The contribution of logical positivists to our clarification of scientific progress was making this very distinction between scientific fields of investigation and non- scientific realms of interpretation. They advanced a razor sharp principle of demarcation leading to a high level of awareness of what constitutes knowledge on methodological grounds. In all epistemological investigations since Plato upto logical positivists, there has been mystifying ambiguity as to the very nature of knowledge. Such a basic methodological ambiguity has always proliferated additional mystifications. Logical positivists brought out that some empirical reference or verifiability is inevitable while considering the epistemic validity of any knowledge-claim. Knowledge and truth are inevitably anchored on a method of verification or confirmation. Such a logical positivistic view has been subjected to large and wide criticism. However, logical positivists have succeeded in redrafting contemporary epistemological discourse (Jamal khwaja, 1965, p.91).

(ii) KARL POPPER: PROGRESS AS FALSIFIABILITY

Twentieth century Australian philosopher of science, Karl Popper (1902- 1994) has made outstanding contribution to the development of the philosophy of science. His point of departure is his rejection of the positivistic criterion of verifiability replacing it by his criterion of falsifiability. He tried to delineate the boundary between science and non- science. The criterion of falsification was one of the fundamental ideas in the philosophy of Karl Popper. For Popper the statements which are falsifiable or potentially disprovable can be regarded as scientific. In view of the same, those propositions which are not amenable to falsification can be regarded to be unscientific. Thus metaphysical and theological propositions are not scientific for they are falsifiable.

The central concern of Karl Popper was to construct a model of scientific rationality that could bring out the logical relationships between theoretical propositions and observation statements in science. Popper was an epistemological fallibilist. According to Karl Popper, the growth of knowledge has always been and presently continues to be the central problem of epistemology. The best way to understand the growth of knowledge is to study the growth of scientific knowledge (Karl Popper, 1959, p.15.). In view of the same, Popper was deeply interested in the providing the line of demarcation between science and non-science or the demarcation between empirical sciences from non-empirical areas of study such as logic, pure mathematics, and metaphysics as well as astrological and phenological sciences. (Ibid.p.49)

In working out a demarcation between sciences and non-sciences, Popper brought out his radical opposition to the logical positivistic criterion of verification or verifiability. Logical positivistic identification of the meaningfulness and the scientific status of the proposition with its verification or verifiability, according to Popper, reduce a real philosophical problem to a trivial verbal one. Logical positivists treat demarcation as a problem amenable to the method of discovery. They presuppose that there is a clear-cut line between science and metaphysics. They ignore that metaphysical theories are capable of developing into scientific theories, the classical case in this regard being the theory of atoms advanced by ancient Greek philosopher, Democritus. Popper tries to furnish a non-naturalistic criterion of demarcation. According to Popper the demarcation between science and metaphysics cannot be a discovery; it has to be drawn by recourse to agreement and decision. Such a decision, of course, must be logically and methodologically informed agreement. (Ibid, p.37.)

Popper not only rejects naturalistic approach to the problem of demarcation as advanced by the logical positivists, he also repudiates their view that science is characterised by its inductive methods and it is through a set of singular statements from which universal laws are supposedly inferred. Popper advances several considerations against the observational and inductive paradigm of scientific investigation. Firstly, there are no 'pure' observations. There cannot be a theory-free observation. There cannot be any observation that does not involve application of theoretical terms. All observations operate within a conceptual scheme. Secondly, scientific laws are unverifiable. No finite number of observation statements can be logically equivalent to or can justify a universal proposition. Thirdly, a scientific law is never inferable from particular experimental results. We can never have a scientific theory whose truth can be demonstrated at the level of experimental testing (Karl Popper, 1965. p.46, 53).

However, Popper advanced the view that a single genuine counter-instance can turn out to be logically decisive. It is not possible to infer a theory from observation statements. However, the possibility of refuting a theory by observation statements remains always there (ibid.p.55). It is always possible to deductively infer the falsity of a universal proposition if we can determine the truth value of a related singular proposition. Thus, for Popper, the crucial inferences involved in science are deductive. We can proceed from an observation report such as, "this A is not X" to the falsity of the corresponding universal hypothesis. In view of the same, Popper's conception of the relationship between scientific theory and our perceptual experience was anti-inductivist. A scientific theory can neither be logically derived from nor confirmed by perceptual experience. For Popper, all human knowledge including the most refined scientific knowledge, is falsifiable as well as hypothetical. Such knowledge is not a function of logical inference but of the creative imagination. Problem-solving is the most crucial rational activity carried out in science. It is through problem-solving process that new hypotheses are imaginatively projected and pervious theoretical frameworks are rejected. The crucial role of experience in scientific research is to demonstrate not the verifiable truth of the theories but their falsifiable nature. A theory that can withstand the critical testing procedures of falsifiability can be deemed to be corroborated. Such a theory can be considered as preferable to its so-called verifiable rival theories (ibid.p.47).

Karl Popper maintains that scientific theories are not arrived at by recourse to induction. A scientific hypothesis is formulated by recourse to creative imagination rather than an exercise carried out by an inductive process. A scientific hypothesis is not a passive reaction to observable regularities. We are not blessed by the power of pure observations. Our observations are always selective. They are always guided or prejudiced by some pre-theoretical standpoint. More importantly, the inductive process can never justify a hypothesis even if it is granted that we arrive at a hypothesis by way of inductive generalisation (Ibid.p.46). The growth of scientific knowledge starts with an imaginative proposal called hypothesis. A hypothesis is a matter of individual and unpredictable insight which insight cannot be appropriated or arrived at by recourse to a set of methodological rules or steps. A good scientific hypothesis is one that excludes some observable possibilities. If we have to test a hypothesis, we apply ordinary deductive logic to it with a view to deriving singular observational statements from it. Thereafter, if a singular observation statement can be falsified the hypothesis itself stands nullified. A rigorous scientific test of a hypothesis will have to sustain a persevering search for negative and falsifying instances. All hypotheses are not equally falsifiable.

Those hypotheses tend to be more falsifiable which exclude more. Consequently they have a greater chance of being refuted. For example," All heavenly bodies move in eclipses" is more falsifiable than "all planets move in eclipses". It is so because all that refutes the second statement refutes the first statement as well. However, on the other hand, that which refutes the first does not necessarily refute the second. The proper method of science is to formulate the most falsifiable hypothesis. The most falsifiable hypothesis has least probability of qualifying to the level appropriate to scientific credibility. Those hypotheses that are simplest have greater empirical content. Secondly, scientists take great pains in their search for negative instances (Karl Popper, 1959, P.50).

For Karl Popper, verifiability is not the basic character of meaningful propositions. Rather it is the falsifiability that fundamentally characterises scientific propositions. The propositions of science can originate from any source: mythological, theological, fictional or metaphysical etc. However, the scientific credibility of any proposition is determined by its falsifiability or capacity to be refuted. Karl Popper does not accept the notion of "manifest truth" as was advocated by Bacon and Descartes. Karl Popper brings out that arriving at an exact or unquestionable truth is not possible. Some beliefs can tenaciously survive for thousands of years and yet they can turn out to be erroneous. In view of this, the epistemology advanced by Bacon and Descartes sounds to be more optimistic than is warranted by the complexity of the situation. The advocacy of manifest truth has often led to fanaticism. The advocates of manifest truth have often underlined that only most depraved human beings can reject manifest truth. However, a manifest truth is constantly in need of interpretation and reinterpretation which process often does lead to authoritarianism. As against the advocacy of manifest truth, Karl Popper is a staunch exponent of radical fallibilistic epistemology. Karl Popper regards the process of trial and error to be the defining condition of man. Those who are in search of truth have to persistently search for their errors through self-criticism. The existing parameters of the truth have to be radically critiqued with a view to gaining a measure of truth. However, it is not man's destiny to reach ultimate or final truth. Our quest for knowledge or truth can never liberate itself from prejudices, dreams, hopes, and fears. It is by persistently going on eliminating errors that we can hope to achieve a measure of truth. Our search for truth is actually our struggle for elimination for errors (Ibid, pp.49-54).

In contrast to traditional methodological account of scientific knowledge, Karl Popper underlines the conjectural origination of scientific assertions, statements and propositions. Our fundamental assertions about the world are not obtained by recourse to inductive generalisations as held by Bacon, Mill and Logical Positivists but rather by conjecture. The Logic of scientific discovery is not inductive. The method of science is rather "Conjecture and Refutation". Inductive generalisation itself is not a logical or analytic principle. Therefore, any statement derived from an inductive generalisation can be refuted without leading to any self-contradiction. Observations can never yield theories which are impossible of refutation. If it were so it would have been logically impossible to refute scientific theories. However, as a matter of fact, scientific theories are constantly revoked in the light of future observations and investigations. More importantly, it is historically wrong to maintain that the scientific theories have emerged from inductive generalisation. They, rather emerge from new interpretation of available data or bold conjectures forwarded by creative geniuses of science. Observations and inductions do not lead to theories. It is rather the theories that lead to observations. Our observations are oriented or coordinated by scientific theories. The accumulations or observations are a function of the growth of scientific theories. New scientific theories lead to new observations which observations may provide further critique of scientific theories. This ongoing process leads to increasing authentication of scientific theories. Karl Popper likens scientific theories to Myth making (Ibid, p.53). Just as myths are the products of imagination so are scientific theories a function of our imaginative or creative ingenuity. When we appropriate a theory, our observations, interpretations and investigations are oriented to that very direction. We observe in the light of our preferred scientific theory. For example, it was Marx's creativity or imagination which forwarded or advanced the theory of class struggle. Thereafter, Marxists have assembled or contrived enumerable data with a view to supporting or authenticating the theory of class struggle. So is the case with Freudians. They observe repression and sublimation everywhere. The followers of Adler observe everywhere or in every action the feeling of inferiority complex. It is the theories which design the observations

(Ibid, p.37.). In view of the same, Karl Popper advocates that nothing can be grounded on pure data for there simply is no pure data. Everything comes to us interpreted or nothing comes to us un-interpreted. What we claim to be knowledge is a function of our interpretation in the light of our theories which theories are nothing but mythical or conjectural projections. When we critically evaluate these theories, they are refuted. The dialectical interplay of conjectures and refutations is the prime source of scientific knowledge. This is the prime method of advancement of scientific research as well. However, the critique or refutation of a conjecture is more often than not grounded upon a considerable amount of common background knowledge. This common background knowledge is simultaneously itself subjected to critical scrutiny. This is the starting point of the critical evaluation of scientific theories, for critical evaluation has to start somewhere. We are living in a highly complex world where there are no guidelines as to where to start and where to stop. We only come to know that there are theoretical traditions and theoretical frameworks which somehow go on operating successfully. The scientific tradition serves as a network or a frame of reference in our ongoing scientific investigations. A constant and continuous critique of our theoretical framework constitutes the scientific progress.

(iii) THOMAS KUHN: PROGRESS AS PARADIGM SHIFT

Thomas Kuhn (1922-1996) is the most widely read and influential philosopher and historian of science of the twentieth century. The philosophy of science registered a Paradigm-Shift with Kuhn's publication of The Structure of Scientific Revolutions in 1962, possibly the most influential book in the history of philosophy of science. The book undermined the logical positivistic and general methodological concerns of the philosophy of science. Kuhn oriented the philosophy of science to look at the history and evolution of science. He advanced a powerful critique of Aristotle's cumulative conception of scientific progress. Rather, he brought out that scientific research was paradigm-bound and registers progress by recourse to Paradigm-Shifts (Stathis and Martin, 2008, p. 203).

Thomas Kuhn advanced the view that science is a puzzle-solving enterprise which shows a cyclical pattern of normal science followed by crises, which crises is followed by revolution and which revolution again lapses into normal science. This kind of science resembles the normal scientific progress as outlined by positivistic accounts of scientific progress. During the course of normal science scientific progress is steady as well as cumulative. The scientific community does not encounter any significant obstacles or anomalies. During the course of normal science, scientists of all levels of skill and sophistication are able to contribute to the development of science. In the course of normal science, scientists share a lot in terms of theory, methodology, experimental equipment, techniques and values. During the period of normal science, scientists question less and accept more. Acceptance of these things is a pre-condition for a professional scientist. The normal science as puzzle-solving is made possible by these very conditions. At this stage, scientists engage in various types of puzzle-solving such as determining the value of constants, equations, perfecting experimental techniques, extending the application of an existing theory to new instances etc. While during these puzzle-solving activities, scientists do not challenge or refute the basic theory which serves as an essential assumption of their research (Kuhn, 1970, p.24).

In course of the pursuit of normal science problems can arise. New observations can show some serious deficiency in the theory. The theory may not be in consonance with fresh observations. New anomalies can crop up which may pose a threat to a theory. However, it may not lead to immediate abandonment of the theory. The anomalies can be regarded mere incentives for puzzle-solving i.e., reconciling the fresh observations with the theory. Sometimes, unsolved anomalies may just be postponed for later consideration. However, a situation can arise when anomalies get out of control and proliferate across the entire field of investigation. If the anomalies go on proliferating and posing a radical challenge to the theory, there is a foundational crisis and the very theory may seem to be indefensible. At this stage, it is difficult for the normal science to go on with its puzzle-solving procedures. In course of time, significant numbers of scientific researchers doubt the very efficacy of the theory and the ongoing puzzle-solving procedures and techniques seem to be too little in the face of an upcoming crisis which entails a radical theoretical reconstruction or replacement. This sets the stage for revolution and more often than not, the old theory is replaced by a rival or a new theory. However, the shift to new theory, according to Kuhn is never a smooth affair. There is fully-fledged resistance from the upholders of the pervious theory. There are no substantial rational considerations which force a theory-shift. The new theory is never capable of solving all the anomalies previously settled by the old theory. The new theory may solve a substantial number of ongoing anomalies but may not be able to accord with the solutions of the old puzzles worked out under the previous theory. So, on a balance sheet of the profit and loss, the alternative theory is never beneficial enough to force a rational decision through which we choose to go with the new theory (Ibid, p.121).

Kuhn also talks of Pre-paradigm science. At this stage, there is hardly any agreement on the basics of sciences among its practitioners. At this stage practitioners of science are often divided into competing schools and are involved in disputes on fundamental issues of theory, metaphysics, method, etc. Instead of solving scientific problems, scientists are often involved into disputes between the schools such that pre-paradigm stage comes to an end only when one of the rival schools succeeds in making a discovery or solves a shared problem in a spectacular fashion. When such a progress is registered in any one of the schools, followers of other schools join them in course of time. Such a discovery becomes the paradigm or the model of normal science, to begin with. Kuhn is essentially a conservative philosopher. He underlines the importance of tradition in shaping out the ideas and practices of working scientists. Doing science, according to Kuhn, is essentially a normative enterprise. The working scientist operates within a system of values, norms and traditions. Kuhn underlines that scientists cannot carry on their research without according a measure of respect to the tradition and culture within which they are operating. A scientific tradition is implicitly and explicitly anchored on a paradigm. Scientific research demands that the scientist respects the paradigm he is operating within. However, scientific research also entails that beyond a point the scientists cultivate the courage to discard the paradigm as well, for in the final analysis all paradigms have their expiry date. Thus, scientific research, according to Kuhn, is an ongoing conflict between tradition and innovation. It is not the case that a single anomaly or a cluster of several significant anomalies do logically refute an ongoing paradigm. In view of the same, there is always room for rational disagreement with regard to refutation of a paradigm in case anomalies arise within the ongoing scientific research. There is also a room for rational disagreement as to whether a new paradigm should replace an older one. In view of the same, scientific research according to Kuhn is often impacted by extra-scientific considerations (Stathis and Martin, 2008, p.70).

In his 'The Structure of Scientific Revolutions', Kuhn's notion of Paradigm became an extraordinarily popular term and has been widely discussed by practioners of natural science, social sciences and human sciences for the last fifty years. For Kuhn, the broader meaning of 'Paradigm' is a consensus around a variety of components of scientific activity; key theories and equations, terminology, accepted mathematical techniques and experimental procedures. Kuhn uses 'exemplar' in the narrower sense of paradigm. An exemplar is a significant scientific achievement in so far as it leads to a puzzle-solution or set of related puzzle-solutions. An exemplar can crystallize support around it which can serve as a model for future research. The proposed puzzle-solutions in the future are evaluated according to their similarity to the exemplar, for there are no rules by the application of which judgments of similarity can be settled. The upcoming scientists are not supplied facts and methodological rules with a view to making discoveries. Instead they are trained in the use of exemplars and techniques through repeated practices. Through this process, the students of science are oriented to procedures involved in various exemplars with a view to installing in them a feel for scientific research. Through an understanding of exemplars the students see new puzzles and new puzzle-solutions and thereby acquire the necessary confidence and creativity needed for being involved in the ongoing scientific research.

Kuhn's 'The Structure of Scientific Revolutions' has had an astounding impact on philosophy of science since its publications in 1962. His explanation of science in terms of paradigms and exemplars provided a radical account of science which was in contradistinction to the conception of science advanced by Logical Positivists who maintain that the confirmation or falsification of a scientific hypothesis is rule-governed. Such a rule can be explicitly written down and followed algorithmically. Such a notion of inference was understood to be a criterion of the rationality of science. On the other hand, Kuhn suggested that confirmation or rejection of a scientific hypothesis is not governed by explicit, formal rules but by a non-formal condition of similarity to an exemplar. Kuhn's critics suggest that Kuhn was advancing the view that science is essentially an irrational enterprise. Many of his critics were convinced that Kuhn was advancing a relativistic view of science for scientific rationality and truth were relative to a paradigm rather than arrived at by reference to some fixed standard. However, Kuhn's intentions were different. Kuhn's self-understanding of his account of science was also different. He underlined that scientific rationality was not what Logical Positivists understood by it. Learning for exemplars was a wide-spread method of learning in diverse fields such as language, history, human sciences and social sciences. Learning by exemplars, therefore, could not be an irrational way of learning or understanding in science.

Kuhn's 'The Structure of Scientific Revolutions' brought out how history of science can be deeply influential on philosophy of science. Kuhn showed that, a purely normative or methodological construction of science is impossible of formulation. Scientific conclusions are at best empirical and can never achieve the status of Logical demonstrations. It is better that we inform prescriptions of the philosophy of science by the descriptions of history of science. A realization of the need for mingling philosophy of science with the history of science led to the emergence of such academic programs in America in 1960's and 1970,s that would pursue both history and philosophy of science simultaneously. Kuhn himself, however, was skeptical about the success of such a project for he thought history and philosophy require two different mind sets. Kuhn always complained that the picture of science forwarded by philosophers was not recognizable to the historians of science.

(iv) IMRE LAKATOS: PROGRESS AS CHANGE IN RESEARCH PROGRAMME (1922-74)

Kuhn's notion of science is distinct not only from logical positivism but also from Karl popper's critical rationalism. Methodological falsificationism, as explicated in Popper's book 'Logic of Scientific Discovery', is the precise contribution of critical rationalism to our understanding of scientific progress. According to Popper a scientist's approach in the direction of chosen hypotheses must not be one of in search of confirming occasions but should be rather one of testing them thoroughly in order to find out possible refuting instances. In the face of a refuting instance the scientist should try to give up the hypothesis and search for a substitute. Scientific progress is a matter of repeated conjectures and refutations, new conjectures and refutations and so on (Lakatos, 1984, pp.90-93).

When set in opposition to Kuhn's emphasis on the implications of normal science, Popper's falsificationism offers a harsh contrast. In Popper's view scientific progress occurs only as a result of the rejection of a hypothesis, while in Kuhn's account the latter occurs only during bizarre science, which is to say as the outcome of a scientific revolution. Thus Popper pays no attention to normal science and regards all progressive science as revolutionary. Moreover Popper regards refutation as a logical matter while Kuhn holds that the rejection of an old paradigm is not logically convincing and may be a matter over which rational divergence is possible, as a result of which a scientific revolution may be a long drawn-out matter. Rather than criticize accepted theories, Kuhnian normal scientists take them as given and seek to fill in any remaining gaps in those theories or to relate them to new phenomena. During normal science any anomalies are characteristically abandoned rather than taken as grounds for rejecting the theory. Only the growth of particularly problematic anomalies - those that present difficulties for the very tradition of normal science – leads us to disbelieve with regard to paradigm theories. According to Popper Kuhnian normal science shows insidious conservation. According to Kuhn Popperian methodological falsificationism fails to match the facts of the history of science (Ibid).

Furthermore Kuhn's critics regarded Kuhn as declaring that science is irrational. Kuhn tells us that revolutionary disputes cannot be decided by rational, logical argument. Kuhn says that: Individual scientists embrace a new paradigm for all sorts of reasons and usually for several at once. Some of these reasons lie outside the apparent sphere of science altogether. Others may depend upon idiosyncrasies of autobiography and personality. Even the nationality or the prior reputation of the innovator and his teachers can sometimes play a significant role (Kuhn, 1970, p.152). This indicates that external factors may play a role within the outcomes of scientific disputes. For Kuhn, the most effective way of advancing a new paradigm is to illustrate that it solves the problem that led the old one into crisis, the role that crucial experiments can play.

Kuhn's critique of rationality led Imre Lakatos to regard Kuhn as taking scientific change to be a matter of 'mob psychology' (Lakatos, 1970, p.178).

However, Lakatos did identify the force of Kuhn's historical criticism of Popper. All significant theories have been bounded by an 'ocean of anomalies', which on a falsificationist view would need the rejection of the theory completely. In his "Falsification and the Methodology of Scientific Research Programmes" (1970) Lakatos wanted to reconcile the rationalism of Popperian falsificationism with what seemed to be its own refutation by history.

The crucial problems of the philosophy of science were taken up by several philosophers during the second half of twentieth century. Karl Popper had provided the basic texts for the debate. With the publication of Kuhn's The Structure of Scientific Revolutions, several problems of the philosophy of science became all the more debatable. Several important philosophers of science attempted a rational reconstruction of scientific progress which led to wide and large discussions about the methodological and substantive issues in the domain of philosophy of science. These discussions were conducted in several directions. Perhaps the most important direction it culminated into was the rational reconstruction of scientific progress carried out by Imre Lakatos (Lakatos, 1970, p.177).

Kuhn's emphasis on continuity in science was greatly appreciated and acknowledged by Lakatos. Historically speaking, scientists have continued using theories in their scientific research even in the face of refuting evidence. For example, scientists in nineteenth century continued to use Newtonian Mechanics, even when several radical anomalies were recognised in it. For Lakatos, such continuity did not constitute or signify irrationality as is made out by Popper while outlining his methodological principles. Logical refutation should not be constituted as methodological rejection. Scientific theories can flourish even in the face of numberless anomalies (Lakatos, 1968, p.151).

However, Lakatos is critical of Kuhn for treating revolutionary episodes as instances of mystical conversion. Lakatos says that Kuhn has unnecessarily depicted history of science as an irrational story punctuated by periods of rationality. Lakatos attempts to furnish a rational reconstruction of theory-replacement. Popper had attempted rational reconstruction, bringing out that scientific progress was an ongoing sequence of conjectures and refutations. Lakatos seeks to improve upon the Popperian reconstruction. Lakatos underlined that the basic unit for appraisal has to be 'Research programmes' in the place of individual theories. A research programme, Lakatos says, firstly, consists of methodological rules. Secondly, some research programmes tell us what paths of research to avoid, thereby, providing us the negative heuristic. Thirdly, some research programmes tell us what paths of research to follow, thereby, providing us the positive heuristic (Lakatos, 1970, p.132).

The negative heuristic of a research programme has the function of isolating a 'hardcore' of propositions, which propositions are not subjected to falsification. Such propositions are accepted and deemed irrefutable by the concerned exponents or practioners of a given research programme. On the other hand, by recourse to the positive heuristics, we develop strategies with a view to constructing scientific theories in such a way that limitations at any stage can be circumvented. The positive heuristic is, for all functional purposes, a set of procedural suggestions. These procedural suggestions are worked out with a view to dealing with anticipated anomalies. When a research programme starts unfolding, in course of time, the hardcore of non-falsifiable propositions are protected by various auxiliary hypotheses. When we test a research programme, the full force of critical evaluation is directed at the auxiliary hypotheses. However, Lakatos underlines that a single negative test result does not invalidate an entire research programme. When we get a negative test result, we can modify the auxiliary hypotheses to accommodate the anomaly. Sometimes, the best available strategy can be to shelve the anomaly for future consideration. An appraisal of a research programme can be carried out against rules of appraisal for sequences of theories. Lakatos qualifies some sequences as 'progressive problem-shifts' and other sequences as 'degenerating problem-shifts' (Ibid, pp.135-6).

A research programme is progressive if its theoretical growth presages its empirical growth. It is progressive if it successfully keeps predicting novel facts. As against a progressive shift, a research programme can also negotiate degenerating shift. A research programme stagnates if its empirical growth accelerates and its theoretical growth degenerates. When a research programme progressively explains more facts than its rival research programme, it can supersede and even eliminate its rival. Within a given research programme, we can replace a theory by a better theory, a theory that has great empirical content in comparison its predecessors. However, it is extraordinarily difficult to exactly pinpoint as to when a research programme outcompetes its rival research programme. Neither a logical proof nor an experimental verification can dislodge a research programme instantly. A better research programme can falsify its rival research programme. However, such a falsification can be declared only with hindsight. Pending falsification, two rival research programmes can exist together at the same time. Even more than two programmes can flourish simultaneously (Ibid, pp.116-18).

A research programme is relatively autonomous. A research programme does not become rejected with the emergence of counter-evidence. As a research programme has some degree of autonomy, it can ignore counter-evidence. Such ground realities make scientific progress rational. A research programme can continue to exist even in the face of large number of anomalies and counter-instances. We can carry on with a research programme by bracketing up the anomalies for a while. It may become rational to carry on with the positive heuristic of the programme for the nature of scientific problems is defined by its positive heuristic rather than by confronting anomalies (Ibid, p.138).

(v) FEYERABEND: PROGRESS AS THEORY CHANGE

Paul Feyerabend (1924-1984) was an eccentric and creative philosopher of science. He was critical of the very possibility of philosophy of science. Besides, he was a critic of positivism as well as falsificationism. He also critiqued all rationalist attempts to prescribe or explore rules of scientific method. He designated his view of science as 'anarchistic', underlining the rejection of the dogmatic use of rules.

Feyerabend was powerfully impacted by Ludwig Wittgenstein and Karl Popper. In his critique of logical empiricist philosophy of science, Feyerabend interpreted scientific theories with refrence to Popperian conventionalism. He argued that the question of theory-choice or theory-replacement is not a dispute to be settled by rational arguments. It is, actually, a matter of choice. It is up to us to decide whether a theory is a description of reality or we can decide to designate it as an instrument of prediction. It depends on our ideas of scientific knowledge and our aspirations of and expectations from science. Such disputes are to be decided by the consequences of theories. Philosophical theories do not merely reflect science but change it as well. Our forms of knowledge can be changed to fit our standards.

According to Feyerabend, there is no pure or unalloyed observation language. Every so-called observation language is grounded upon a meta-physical ontology. Furthermore, the theories we hold influence our language and even our perceptions. So long as we feel committed to a particular theory, we become incapable of envisaging alternative accounts of reality. Thus a scientific theory may also be, in course of time, reduced to an irrefutable dogma or myth (Feyerabend, 1981, p.2).

Feyerabend is a defender of realism. According to him, "the interpretation of a scientific theory depends upon nothing but the state of affairs it describes" (Ibid, p.2). He acclaims Wittgenstein's Philosophical Investigations according to which book the meaning of terms is determined by the role they play in the wider context of a theory. It is our theories which determine the interpretation of an observational language. It is through theories that we try to explain our observations. If the theories change our interpretations and explanations also change. Realism interprets theories as universally quantified statements. Thus it leads to scientific progress rather than stagnation. According to Feyerabend, only realism allows us to live upto the highest intellectual ideals of critical neutrality, honesty and testability (Ibid, p.31). For Feyerabend realism is desirable because it demands the production of new and incompatible theories. When new incompatible theories are proliferated the empirical content of the theories is enhanced. A theory's testability is relative to the number of potential falsifiers it has. It is through production of alternative theories that we can certify the existence of possible falsifiers. Scientific progress is a function of theoretical pluralism. When we allow a plurality of incompatible theories, the competition thereof will maintain an even enhance the testability and also the empirical content of the theories. When various theories are tested against one another, all of them are forced to develop through competition.

According to Feyerabend, all our statements beliefs and experiences are hypothetical. Our experiences and observations always need interpretations. It is through different theories that we appropriate or arrive at different interpretations. All observational statements implicitly embody theoretical principles such an exercise requires us to change the meanings. Thus all words operate within a diffused framework of semantic instability instead of semantic stability presupposed by positivistic accounts of reduction, explanation and corroboration. Progress in science demands violation of semantic stability. If meaning is determined by theory, terms in very different theories cannot share the same meaning. If we want to derive the principles of an old theory, from those of a new, one must either not succeed at all or must negotiate a change in the meaning of the terms of the old theory. Any theoretical reduction is more like replacement of one theory and its ontology by another. In view of the same any formal account of explanation or corroboration is precluded by semantic instability. Feyerabend has brought out a radical rejection of the idea that scientists are rational rejection of the idea that scientists are rational creatures and nothing more. In his celebrated book 'Against Method', Feyerabend argues that scientists do not in fact follow any one given method. They are less rational then their propaganda constitutes them to be. There are no exceptionless methodological rules governing the progress of science. There are no rules governing the growth of knowledge. The history of science is complex. We cannot develop a general methodology with regard to scientific research. The only method that is corroborated by history of science is 'Anything goes'. If we follow logical empiricist methodologies or Popper's critical rationalism, we would inhabit scientific progress by imposing restrictive conditions on new theories. Imre Lakatos does not fear any better than logical positivists or Karl Popper. His methodology of Scientific Research Programmes is full of ungrounded value judgments about what constitutes good science. His methodology is reasonable to the extent it is disguised epistemological anarchism. It was Feyerabend's life-long ambition to undermine the so-called privileged status of science within contemporary global culture. He argued that there is no scientific method justifying science as the best way of acquiring knowledge. Science prevails only because "the show has been rigged in its favour" (Feyerabend, 1978, p.102). Feyerabend brings out that science is far closer to mythology than can be admitted by philosophers of science. It is not necessarily the best form of thought developed by man. Its conspicuousness, noisiness and impudence do not necessarily make it inherently superior. Only those who have uncritically accepted science and its ideology can deem science to superior to other forms of knowledge (Feyerabend, 1975, p.295).

Feyerabend recommends that just as we have separated church from state, so we should separate science and state. We need to setup a free society in which every tradition of knowledge is equally respectable and equally proximate to the centres of power (Feyerabend, 1978, p.9). As Feyerabend sees it science is a threat to democracy. We ought to control science through democratic power. We also ought to cultivate intense scepticism with regard to scientific experts. Such experts too must be accountable to and controllable by democratic institutes.

(vi) STEPHEN TOULMIN: PROGRESS AS RATIONALITY

Toulmin (1922-2009) is a twentieth century philosopher of science. In his book "The Philosophy of Science" first published in1953, Toulmin tries to explore the nature of arguments in the science. He especially laid emphasis on the question of the rationality of the scientific discovery. Toulmin has provided an anti-positivistic account of the rationality of science. He has underlined that scientific arguments as well as the arguments advanced in many other fields except the pure sciences are impervious to description or explanation in terms of formal logic. The specific character of scientific enterprises can neither be captured by deductive nor inductive methods of inference. It is especially the case with those scientific practices or enterprises that culminate into new discoveries. Toulmin brings out that the basic or fundamental purpose of science is not the discovery of the objective true knowledge. Science rather tries to furnish us with new foresights of understanding of the world through a relevant theory. For example, Toulmin characterizes physics as presenting a new way of regarding old phenomena. The scientific discoveries do not necessarily reveal new or unknown facts. A scientific breakthrough is essentially rather an interpretation in a new or different way of what we are already aware of (Toulmin, 1967, p.16).

According to Toulmin, in scientific investigations or discoveries, the pivotal rule is played by the method of representation (Toulmin, 1967, p31). The method of representation means a sort of graphical, pictorial image of a given physical phenomenon. The mathematical models perform this role on a more sophisticated level. According to Toulmin such pictorial, graphical or geometrical models were widely worked out during the early history of science. The community of scientists during early centuries of modern sciences was deeply oriented towards the acceptance of proffered scientific solutions by recourse to conventional graphical representations. Various scientific fields of investigation such as optics, physics, chemistry etc can be shown to have been historically deeply impacted by geometrical and trigonometrically models. Toulmin suggests that even very abstract mathematical models play a role similar to the pictorial methods of representation. They may be said to be counterparts of geometrical images (Toulmin, 1967, p.32).

Toulmin stresses that scientific investigations of nature are not carried out by scientists as natural unmediated observers; rather they are equipped with some fundamental intuitions with regard to the ways of representing the phenomena under consideration. They try to employ those representational methods better suited in terms of their explanatory power. Toulmin underlines that when physics claims that "Heat is a form of motion" or that "Light travels in straight lines" or that "X-rays and light-waves are varieties of electro-magnetic radiations", he is not stating the pure facts or discovering anything which can factually ascribed to reality. Any scientific discovery is worked out or appropriated through a given method of representation. Toulmin writes:

"The heart of all major discoveries in the physical sciences is the discovery of novel methods of representation, and so of fresh techniques by which inferences can be drawn and drawn in ways which fit phenomena under investigation" (Toulmin,1967, p31).

Toulmin's ideas about scientific discoveries do contribute to the way we understand scientific discoveries and arguments. As a philosopher of science, he brings out the limits of methodology, especially its formal methods. It is Toulmin's conviction that formal methods cannot be of great help while trying to understand the nature of scientific practice. According to Toulmin, the scientist has to go beyond methodology and formal logic with a view to doing science. A scientific discovery is not a function of methodological instructions or formal algorithms. It is rather a function of professionally trained imagination that is able to negotiate any real breakthrough. In the final analysis, the scientists have to go beyond the formal procedures. A scientist may use highly sophisticated and impeccable computational techniques and yet a given model may not be applicable in a given context culminating in erroneous reasoning.

Toulmin compares scientific reasoning with juristic reasoning. Like jurisprudential reasoning, scientific reasoning also leads to results by achieving consensus within a community of scientists. Scientific reasoning cannot be applied with the principles of logic. Scientific reasoning like legal reasoning is not automatically applied as such but requires a thorough examination of adequacy of the rules with reference to the specific character of a given situation. Like legal regulations, scientific laws too are not universal by definition but need to be suspended or changed in the light of given circumstances. The situation of a scientist resembles the situation of a judge for both of them negotiate cases or disputes which are complex or are quite new and cannot be successfully explained by pervious rules and methods. But the scientists and the judges, in such cases, resort to informal modes of reasoning. They weigh reasons and ponder over the significance of the complexities involved in their respective fields of operations. When they face a problem which cannot be explained in terms of available rules and laws they have to go beyond a current established system. Sometimes specific situations entail the revision and redefinition of the whole old paradigm. Thus a new scientific paradigm and a new legal paradigm may emerge in the hands of a creative scientist or a creative jurist.

Toulmin develops a critique of the procedures of formal logic and especially the rules and cannons of formal rationality. He challenges the relevance of these rules of rationality in arriving at new and significant discoveries. Each science, according to Toulmin, develops its working logic independently of the universal and idealised logic (Toulmin, 1958, p.146).A scientist basically works with the methods of representation which are not universal by definition. In view of the same, it is always reasonable to qualify our arguments and conclusions.

CHAPTER – 3

Larry Laudan's: Theory of Scientific Progress

LARRY LAUDAN'S THEORY OF SCIENTIFIC PROGRESS

I. Research Tradition:

Laudan's (1941) most important contributions to the philosophy of science can be found in his book *Progress and its Problems* (1977). In *Progress and Its Problems* (1977), Laudan rejects several time-honored assumptions of previous philosophy of science, including the notions that scientific progress requires (1) a fixed methodology; (2) the cumulative retention of the successes of earlier theories; and (3) a convergence on "the truth." Laudan argues that since we have no way of determining to what extent our scientific theories about unobservable entities are correct, it is irrational to believe that any of our theories are even partially true. Laudan, unlike Paul Feyerabend, does not abandon scientific realism for relativism after denying that we can justifiably make any claims about theoretical truth. For Laudan, scientific realism only requires us to accept the practical benefits of using theoretical terms in a realist manner. It requires us accepting semantic realism only. However, scientific realism does not require believing that any of those terms actually corresponds to some reality. Nor does scientific realism receive support from the abductive argument, popularized by Hilary Putnam in the 1970s, that only a scientific theory's approximate truth could explain its practical success. As Laudan points out in "A Confutation of Convergent Realism" (1981) and *Science and Hypothesis: Historical Essays on Scientific Methodology* (1981), most past scientific theories enjoyed much empirical success without being true at all; and many scientific theories might, for all we know, actually be close to the truth without enjoying much practical success. Scientific realism cannot be defended by the principle that "if a scientific theory is true, then it will be successful," any more than a simplistic pragmatism could be defended by the principle "if a scientific theory is successful, then it is true."

Broadly speaking, Larry Laudan makes a distinction between two types of theories. More often than not, we use the term 'theory' with a view to denoting a specific set of doctrines. In such a context, the theory can be utilised for making specific experimental predictions. A theory in this sense does also provide a detailed explanation of natural phenomena. Such theories as Maxwell's theory of electromagnetism, Einstein's theory of photoelectric effect, Marx's labour theory of value, Wegener's theory of Continental Drift, and the Freudian theory of the Oedipal Complex etc can be cited as examples of this type of theory. On the other hand, the term 'theory' also refers to general, hardly testable, sets of doctrines or assumptions. For example, theory of evolution does not refer to a single but to a whole spectrum of theories. It refers to an entire family of doctrines that are historically and conceptually related. Laudan cites, 'quantum theory' as an instance of a theory which refers to a large set of doctrines. 'The quantum theory' includes quantum field theories, group theories, s-matrix theory and renormalized field theory. Such a theory is christened by Laudan as 'Scientific Research Tradition'. A specific theory is provided by the research tradition. While a specific theory touches an empirical problem with a view to solving it, answers to empirical problems come from the research tradition. The solutions of the problems emanate from a research tradition. It is through the research tradition that we understand the locus standi and modus operandi of any problem under consideration. We can locate a problem within a research tradition. We can work out a solution within a research tradition (Laudan, 1977, p.71-2).

Understanding entails the ontological and methodological dimensions of the problems under consideration. A research tradition provides such an ontological and methodological understanding. The development of specific theory takes place within a set of guidelines furnished by a research tradition. The guidelines can be ontological i.e. understanding the types of entities within which a research tradition is embedded. The specific theories within a research tradition try to explain all the empirical problems within the ontological framework of the research tradition. For example, if we are operating within the research tradition of behaviourism, only legitimate entities which behaviouristic theories can postulate have got to be physical and psychological which are directly and publically observable. If we are operating within a Cartesian research tradition, the theories have got to be about mind and matter only. Other types of entities are unacceptable within a Cartesian research tradition. A specific research tradition also brings out different modes by which entities under consideration can interact. For instance, entities within Marxist research tradition can only interact by virtue of the economic forces influencing them (Ibid, p. 79).

A research tradition often specifies and stipulates legitimate methodological procedures within that tradition. The methodological principles can accommodate experimental techniques or theoretical testing and evaluation techniques or any other appropriate mode of understanding the domain of scientific research. Thus a scientist working in Newtonian research tradition will have strong inductivist biases. He will espouse only those theories which have been inductively arrived at form an appraisal of the relevant data. On the other hand, a behaviouristic psychologist would go in for the operationalist mode of procedure. Thus a given research tradition is comprised of certain ontological and methodological guidelines. One can repudiate a particular research tradition by abandoning the metaphysics and methodology of that tradition. One can break from the ontology and methodology of a given research tradition. Such deviations or violations are not necessarily unwelcome in the account of scientific progress advanced by Larry Laudan. Such deviations and violations have, at times, inaugurated revolutionary break-through in the history of scientific thought. He, however, stipulates that when a scientist tries to solve any empirical problem, he must be operating within the framework of some research tradition. It is so because we understand a scientific problem within a given research tradition. We just cannot have any good or bad understanding of a problem under consideration without locating it within the context of a given research tradition. Accordingly, Laudan would define a research tradition as follows; a research tradition is a set of general assumptions about entities and processes in a domain of study, and about the appropriate methods to be used for investigating the problems and constructing the theories in that domain (Ibid, p.80-1).

Broadly speaking, a research tradition is comprised of two vital components. Firstly, a research tradition advances a set of beliefs about what sorts of entities and processes make up the domain of enquiry. Secondly, it advances a set of epistemic and methodological norms. Or we can say it brings out as to what is to be investigated and how it is to be investigated; what is the object of scientific investigations and how theories are to be tested and data to be collected etc. However, research traditions are immune to direct testability. Firstly, their ontologies are so general that they cannot yield any specific predictions. Secondly, their methodological rules and norms do not pertain to matters of fact and furnish us any testable assertions. However, a research tradition can be useful in various ways. Firstly, it can indicate a set of assumptions which are beyond controversy and constitute background knowledge to all scientists operating in that tradition. Secondly, it can help in identifying those components of a theory which pose trouble and need to be modified. Thirdly, it can stipulate rules with a view to collecting of data and testing of theories. Fourthly, it can advance conceptual difficulties against any theory which derelicts from the ontological and epistemic assumptions of a given tradition (Hacking, 1981, p.150-51).

Any research tradition is engaged with empirical problems by recourse to formulation of specific theories. Any given specific theory advances an explanation of an empirical problem, in the process, facilitating a solution to the problem under consideration. However, specific theories themselves are furnished by the research tradition. We need to appreciate that our understanding originates from the tradition. Our specific theories are also provided by the tradition and we engage with empirical problems by recourse to these very theories. Such a modus operandi sometimes culminates into the solution of the problems under investigation.

Every research tradition is associated with various specific theories. Every specific theory is designed to illustrate the ontology of the research tradition. Each specific theory will be empirically testable. It will entail some precise predictions about the phenomena in any given field of research. As against theories, a research tradition is not either explanatory or predictive or testable. The entire role of a research tradition is to furnish us with the crucial tools for solving empirical as well as conceptual problems. The more successful a research tradition is, the more adequate solutions of empirical and conceptual problems it does offer (Laudan, 1977, p.81-2).

Laudan brings out that specific theories are not necessarily entailed by a given research tradition. Similarly, research traditions too are not necessarily entailed by the specific theories. At the most, a research tradition can prescribe a general ontological and methodological framework with a view to solving given problems pertaining to any domain. A specific theory, however, illustrates a specific ontology. It also articulates a number of specific and testable laws about nature. If we are told by nineteenth century mechanical research tradition that heat is simply a form of motion, we shall neither deductively conclude Boltzmann's version of the Kinetic theory of gases nor arrive at statistical thermodynamics. Similarly, it is impossible to deductively derive a research tradition from all the theories thereof.

According to Laudan, there are two specific modes indicating the relationship between theories and the research tradition. One mode is historical and the other is conceptual. Historically speaking, most scientific theories have been formulated when the scientists concerned were operating within a specific research tradition. If we go through a historical development of various theories, we can always trace the research tradition with which a specific theory was allied to. Tracing such a relationship must always be considered as one of the most important facts of the past (Ibid, p.86).

Nevertheless, a research tradition impacts its constituent theories in various ways:

Firstly, a scientific problem is determined by the research tradition. A research tradition strongly impacts the formulation of the scientific theories. It influence's the range and weight of the empirical problems which specific theories will have to negotiate with. Our decision as to what can count as genuine problems for specific theories will be oriented by the ontological position and the methodological guidelines of the research tradition. It is the research tradition which figures out the relevance of empirical problems in a particular field of research. It is for the research tradition also to decide which problems fall outside of a given field of research and which problems are pseudo-problems. Laudan brings out that Cartesian mechanical research tradition deeply transformed the domain of optical theories in the seventeenth century. Previously, problems pertaining to perception and vision had been regarded as legitimate empirical problems for any optical theory. The Cartesian tradition argued that such problems do not genuinely belong to the domain of optical theories. They should be relegated to such disciplines as psychology and physiology. Thereafter, such empirical problems were simply ignored by the mechanistic optical theorists (Ibid, p. 86-7).

Secondly, Laudan underlines that the function of a research tradition is the establishment of an ontological vision and methodological guidelines which can tackle with the problems in a given domain of research. In this way a research tradition operates as a constraint on the theories. It can provide us the clues as to which problems can be developed within a given research field. For example, if a research tradition is strongly inductivist or observationlist, it cannot accept specific theories postulating unobservable entities. In nineteenth century, various subtle fluid theories and atomic theories etc were strongly opposed because the then dominant methodology did not approve of the epistemic and scientific soundness of various theories dealing with unobservable entities (Ibid, p.89-91).

Thirdly, the heuristic role of a research tradition cannot be underemphasised. A research has also a heuristic role to play with regard to construction of specific theories. The specific theories are not deducible from the research tradition. However, a research tradition can furnish vital clues for theory construction. Laudan brings out that before developing a theory of light and colours, Descartes specified that bodies have only such properties as those of shape, size and motion. It is true that he did not specify the shapes, sizes, positions and motions of particular bodies, although he categorically clarified that all specific physical theories would have to specifically negotiate only with these four parameters. In view of the same, Descartes was aware that his optical theories would have to be negotiated across such parameters. Accordingly, he tried to explain colours in terms of the shape and rotational velocity of certain particles. His research tradition clearly assumed that the particles of light are exactly like other material bodies; he was convinced that his theoretical analysis of light could benefit by the application of general mechanical theories.

A research tradition can play another heuristic role. Such a heuristic role becomes all the more clear when constituent theories have to negotiate modification. Any research tradition worth its salt will have to have crucial guidelines about modification of theories, which can improve its problem solving efficacy. For example, to begin with, Kinetic theory of gases negotiated crucial predictive failures. However, the research tradition was enormously flexible. Such flexibility paved the way towards possible modifications. Kineticists introduced molecular spin to alert their assumptions about molecular elasticities., with a view to accommodating seeming energy losses by greater degree of freedom. Such strategies become possible by regarding matter as possessing a molecular and mechanical composition. The theoretical anomalies are resolved by such a heuristic role of a research tradition (Ibid, p.92).

Fourthly, rationalisation or justification of theories is one of the most crucial functions of a research tradition. More often than not, specific theories make assumptions about the nature which are not fully warranted by the supporting data. Such assumptions are mostly about basic causal processes and entities. The existence and operation of these processes and entities are taken for granted by specific theories. Laudan stresses that only a research tradition can work out a justification of such assumptions. For example, Sadi Carnot while developing his theory of steam engine presupposes that no heat was lost in performing the work of driving a piston. This assumption was not rationally justified by Carnot. He did not feel a need to do that. It was the primary postulate of the Caloricist research tradition that heat was always conserved. In view of the same, Carnot presupposes certain things about his theory which the theory itself could not establish. It is the general assumption of the Caloricist research tradition that heat is never lost in whatsoever circumstances. In this way, by playing a heuristic role, a research tradition can significantly promote our understanding of scientific problems. Such an understanding can significantly promote the solution of the problems under consideration as well (Ibid).

Fifthly, a research tradition provides us the rationale of theory choice. The aim of science is the resolution or clarification of problems. Another crucial implication of this account is that scientific progress is not parasitical upon rationality. Historically speaking, rationality has been understood as a temporal concept. It was assumed that we can determine the rational credentials of statements or theories independent of any knowledge of their background or evolution. Scientific progress is deemed to be fully dependent upon the rationality. Progress implied adherence to an ascending order of rational beliefs. The problem with such an account of science is that progress is readily understandable whereas rationality is not that clearly identifiable.

II. Problem-solving Effectiveness:

Science is not an aim less enterprise. Its aim is problem-solving. From this perception, the rationality of theory-choice is problem-solving effectiveness. Such rationality is not transcendental to scientific activity. It is imminent in the very scientific activity itself. If, scientific advancement can be defined in terms of problem-solving, then such advancement can be said to be the rationale of theory-choice. It means, progressiveness is determining rationality rather than rationality determining progressiveness. If a problem can be effectively solved by a theory, then such a theory is rationally acceptable. However, when we say that a problem has been solved? The answer to such a question is not independent of the research tradition within which a scientist or a group of scientists are operating. It is the research tradition which stipulates the nature and status of the problems. It is the research tradition which informs us about rules of appraisal and about relevant methodological criteria. With the change of a research tradition, the change of rationality of theory choice also takes place. Here the rational is progressive and progressive is rational.

Theories which are effective in problem-solving can be said to be progressive and rationally acceptable. In such an account of science, the rationale of a theory choice is problem-solving effectiveness.

Determinate the 'problem-solving effectiveness' of a theory is simple. We measure such effectiveness by recording the problems solved by a particular theory in comparison to other competing theories. At this point, Laudan classifies problems into three types; solved problems, unsolved ones, and anomalous instances. It is the solved problems which determine whether a theory is effective than its rival. Only by the record of solved problems can we measure effectiveness of the theory. However, unsolved problems should not be deemed to be insignificant, for it is unsolved problems that are transformed into solved ones. However, in appraising the relative merits of theories, unsolved problems are irrelevant for it is only the solved problems that matter with regard to theory evaluation (Ibid, p. 21-22).

Now that we assume that problem-solving effectiveness is the aim of science, we can shift our attention to the model of progress. Such a model, according to Laudan appropriates certain simple assumptions: (1) The solved empirical or conceptual problems constitute the basic unit of scientific progress; (2) Science aims at maximization of the scope of solved empirical problems and minimization of the scope of anomalous and conceptual problems. If a theory claims to have problem-solving effectiveness, it will have to be determined by its record of solved problems in comparison to unsolved ones. The overall problem-solving effectiveness of a theory is determined by assessing the number and importance of the empirical problems which the theory solves and reducing the number and importance of anomalies and the conceptual problems which the theory generates. The effectiveness of a theory can also be a function of the modification of the theory under consideration. If a theory can illuminate some troublesome anomalies or resolve some conceptual problems, its effectiveness can be said to be registering the highest scale. As the aim of science is problem-solving, and the progress can be registered only if successive scientific theories register an increasing degree of problem-solving effectiveness (Ibid, p. 69).

A specific research theory may at times be progressive and at times non-progressive. In comparison to such theories, a research tradition (RT) is a productive one. We can hold a specific research tradition accountable with regard to the number of specific theories. In view of the same, only a research tradition can have the character of effectiveness. Thus, 'problem-solving effectiveness' of a theory means 'problem-solving effectiveness' of a research tradition. We can replace one (RT) by another (RT). Accordingly, we can say that a (RT) is the unit of scientific change. Only an ongoing research tradition can register important and substantive changes.

Thus, the 'problem-solving effectiveness' has actually to be the rationale for a tradition choice. The reason of such a theory or tradition choice has to be relative to the milieu. A given research tradition does also accommodate the ongoing or contemporary conceptual problems. Laudan claims that it was not irrational of Aristotle in forth century B.C to claim that physical science ought to be subordinated to or be legitimated by metaphysics. Thomas Aquinas was not stupid or prejudiced while espousing the view that science ought to be compatible with religion. The view that science is not independent of religion or metaphysics itself constitutes a research tradition. In view of the same, it may be perfectly rational in twentieth century to appropriate the very same view. However, if a belief is rational in our times or in our age, it does not necessarily entail that it was rational at other times and places. More often than not, quite the reverse results might be arrived at. There simply cannot be a research tradition which can claim to be capable of dissolving the problems universally and eternally (Ibid, p. 131).

Making a choice between two theories entails that these two theories under consideration are comparable with each other. We can apply the rationale of theory choice only if theories under consideration are comparable. As the ontological and methodological assumptions are supplied by the research tradition, we just cannot have a theory-free observational language. How can then, two research traditions be deemed to be comparable? For Larry Laudan, scientific theories can operate without the buck up of the observational language or correspondence rules. However, there is a genuine problem as to how we can demonstrate that different theories actually are dedicated to resolving the same or similar problems?

Larry Laudan provides a straightforward answer to this question. According to Larry Laudan, the terms in which a problem is characterized will generally be dependent upon the acceptance of a range of theoretical assumptions. Such assumptions can or cannot constitute the theories leading to the solution of the problems under consideration. If a problem can be characterized only within the language and framework of a theory that claims to solve it, then categorically it can be brought out that no competing theory could be said to be solving the same problems. However, there can be situations where the theoretical assumptions characterizing the problem under consideration are different from the theories that attempt to solve it; it can be safely assumed that the competing explanatory theories are addressing themselves to the same problem. Larry Laudan brings out that scientists have been reflecting since ancient times to explain why light is reflecting off a mirror according to a regular pattern. This problem of reflection involves many quasi-theoretical assumptions, such as, that light moves in straight lines, that certain obstacles can change the direction of a ray of light, that visible light does not continuously fill every medium etc. There is no necessary inconsistency between the problem-solving theories and those theoretical assumptions required to solve the problem. In seventeenth century, several conflicting were theories of light addressing them to the problem of reflection. All optical theories were deemed to be solving the problem of reflection.

Laudan suggests that scientific problems can be solved by paying attention to shared problems which competing research traditions do encounter. The shared problems provide a basis for rational appraisal of the relative problem-solving effectiveness of competing research traditions. Sometimes, two theories agree to expect some particular problems as a solved one.

Sometimes, it can happen that a problem may be considered as solved one by one theory but not by another. The question that arises can be as to how we can measure the relative merits of two theories. Laudan responds to this question by saying that since rationality consists in accepting the research tradition having highest problem-solving effectiveness, and appropriate determination of the effectiveness of a research can be made within the research tradition itself and not with reference to any other tradition, our criteria or norms will be determined by observing whether a research tradition has solved the problems that it set for itself or whether it generated any anomaly or conceptual problem while solving a problem. Or whether in course of time, it has expanded the domain of solved problems and minimised the scope of anomalies. Laudan is hopeful that if we adopt this procedure and observe the functioning of all basic research traditions, we should be able to construct a progressive ranking of all research traditions at a given point of time. Laudan is thus convinced that it is possible, in principle, to be able to compare the progressiveness of different research traditions even when the traditions under consideration are radically incommensurable in terms of the fundamental truth – claims they advance with reference to the universe (Ibid, p.143-46).

III. Changing Research Tradition:

Laudan brings out that change of a research tradition takes two distinct forms. Firstly, a research tradition can change by a modification of some of its subordinate or specific theories. Such changes are registered within research traditions on a continuous or ongoing basis. It often happens that researchers discover within the framework of the tradition a more effective theory for dealing with some of the phenomena. They can realise that slight alteration in pervious theories or modification of boundary conditions or proportionate revision of constants or minor terminology refinements can improve on the problem-solving effectiveness of any of the theories within the research tradition. Once a more significant or improved theory is discovered, the preceding theory is immediately dropped. In this way, any progressive research tradition, exhibits a long fluctuation of specific theories. Secondly, a research tradition evolves by change of some of its basic or core elements. A look at great research traditions in the history of science such as Aristotleanism, Cartesianism, Newtonianism etc will reveal that none of these traditions did have any interesting set of doctrines. For example, several Cartesians repudiated the identification of matter and extension. Huygens admits the possibility of void space, yet he remains a Cartesian (Ibid, p.96-7). However, more importantly, in course of scientific development, a research tradition itself changes. The ongoing research traditions and the theories advanced by them face numerous problems. New anomalies are discovered and fundamental conceptual problems are encountered. The advocates of an ongoing tradition find it difficult or even impossible to illuminate such anomalies and conceptual problems by recourse to modification of specific theories within the tradition. Laudan says that in such a situation, the proponents of a research tradition try to explore what minimal changes in the methodology and ontology of the tradition can illuminate the anomalies and conceptual problems. However, it is not always possible to square out the problems by recourse to minimal short-cuts. At times, scientists find it impossible to water down any of the assumptions of a research tradition with a view to illuminating its anomalies and conceptual problems. At this stage, scientists have the choice to abandon the tradition. However, the scientists fight to the maximum possible extent and try to solve the anomalies and conceptual problems of the tradition by introducing one or two modifications in the core assumptions. Thus, they try to preserve a research tradition to the extent it is possible (Ibid, p. 98-9).

According to Laudan, a research tradition can negotiate a natural evolution. Such an evolution is neither refutation of the formal tradition nor the creation of a new tradition. A research tradition may negotiate numerous evolutions in its historical development. Such ongoing evolutions over a period of time can eventuate numerous discrepancies between the methodological and ontological dimensions of the earlier and later formulations. Michael Faraday radically reformulated Newtonian research tradition. Sometimes we may not see any degree of similarity between earlier and later stages in the development of a tradition. Nevertheless, it may remain the same research tradition.

Now the question arises as to how be us to understand the replacement of one tradition by another if the tradition continues to be in course of historical development? Laudan states that at any given time certain elements of research tradition are more central to, and more deep-rooted within the tradition than other elements. It is these more central elements, which are taken, at that occasion, to be more characteristic of the research tradition. To abandon them is indeed to move outside the tradition, while the less central tenets can be modified without repudiation of the research tradition. But what are the unrejectable elements and how are they rejected? He says, when it can be shown that certain elements, previously regarded as essential to whole enterprise, can be abandoned without compromising the problem-solving success of the tradition itself, these elements cease to be a part of the unrejectable core of the research tradition (Ibid, p. 99-100). So, it is the old tradition out of which the new emerges. The old one changes into the new tradition. Research traditions differ with one another. New traditions emerge out of old in course of historical evolution. All successive stages do have continuity during their ongoing march.

We can raise the question as to how a theory can respond in the face of an anomaly. Can we say that a break has been registered in the change and continuity of a theory? According to Laudan, the happening of an anomaly raises doubts about, but need not compel the abandonment of theory exhibiting the anomaly. For him, this is because of two reasons: (1) in any empirical test, it is an intricate combination of a variety of theories that is essential for deriving any experimental prediction. For instance, in order to test a theoretical statement as simple as Boyl's law, we must raise queries about the behaviour of our measuring instrument. Boyl's law by itself predicts nothing whatever about how these instruments will behave. Now in this condition, if the prediction turns out to be incorrect, we do not know where to locate the error within the complex. It follows that we can never rationally claim that any theory has ever been refuted (Ibid, p.41). We cannot decide that one particular theory within the network is completely false. On the other hand, to abandon a theory because it is incompatible with the data assumes that our knowledge of the data is infallible and veridical. But once we realize that data itself is only probable, the occurrence of an anomaly does not necessarily require the abandonment of a theory. (2) One of the most cognitively significant activities of scientists is the successful transformation of a presumed empirical anomaly for a theory into a confirming instance for that theory. There are many instances in history of science that anomalous instance has turned out to be the confirming one. For example, Prout's view was that all the elements were composed of hydrogen and consequently the atomic weights of all elements should be integral multiples of the weight of hydrogen. Berzelius and others found that several elements had atomic weights incompatible with Prout's theory. These results constituted very serious anomalies for Proutian chemistry. But discovery of isotopes and refinement of techniques enabled physical chemists to separate out the isotopes of the same element. Each isotope was found to have an atomic weight that was an integral multiple of hydrogen. Previous anomalous results could now be explained on Prout's hypothesis. Thus, the very phenomenon that had earlier constituted anomalies for Prout's hypothesis becomes positive instance for it (Ibid, p.24-31). That is why Laudan says, a scientist can adhere to a theory of less effectiveness in the hope that he will improve it. He holds that the choice of one tradition over its rival is a progressive choice precisely to the extent that the chosen tradition is a better solver than its rivals. He entitled this sort of appraisal the context of acceptance. A further mode of appraisal is context of pursuit. There are many historical cases where scientists have investigated and pursued theories or traditions that were potentially less acceptable, less worthy of belief, than their rivals. Scientists often begin to pursue and explore a new research tradition long before its problem – solving success qualifies it to be accepted (Ibid, p.109-10). Prout did not leave his theory as he saw it in anomalies. He was rationally justified in doing this, for a scientific theory aims at problem-solving and Prout did not leave his theory as he saw it in anomalies. He was rationally justified in doing this, for a scientific theory aims at problem-solving and Prout also aimed at problem-solving while adhering to the theory. His adherence to the theory culminated into a very important progress of science. Registration of progress does not definitely mean repudiation of a theory. Researchers can have their rational justification for the espousal of an old scientific view.

Such rationality, according to Laudan, is precisely what is known in scientific parlance as 'promise' or 'fecundity'. Daltonian atomism generated so much interest in the early years of the nineteenth century largely because of its scientific promise rather than its concrete achievement. At Dalton's time, the dominant chemical research tradition was concerned with elective affinities. But Dalton's early atomic doctrine could claim nothing like the overall problem-solving success of elective affinity chemistry; still worse Dalton's system was confronted by numerous serious anomalies. But this does not entail that atomists shall not be able hereafter to explain these apparent anomalies in satisfactory manner. Although most scientists refused to accept the Dalton's approach, many nonetheless were prepared to take it seriously, claiming that the serendipity of the Daltonian system made it at least sufficiently promising to be worthy of further development and refinement. Sometimes, a research pursuit can appear to be regressive. However, there are grounds to suggest that it can always be a rational pursuit (Ibid, p.111-14).

According to Laudan, a scientist can also pursue his research in two research traditions with a view to accelerating the problem-solving effectiveness of his pursuit. There are two ways in which different research traditions can be integrated. In some cases one tradition can be grafted onto one other. Thus, in eighteenth century natural philosophy, many scientists were simultaneously Newtonians and subtle fluid theorists. Their adherence to the research tradition of subtle fluid led them to postulate imperceptible aetheria fluids in order to explain the phenomena of electricity, magnetism, heat, etc. Their Newtonianism led them to assume that the constituent particles of such fluids interacted not by contact but rather by means of strong forces of attraction and repulsion, acting-at-a-distance across empty space. These two major researches fused into another research tradition, which is christened as materialistic framework of research. Therefore, it is highly erroneous to assume that a working scientist cannot operate within a couple of research traditions simultaneously.

However, there are problems which must be faced squarely. While mingling two or more than two research traditions, we need to repudiate some of the fundamental elements of each of the traditions under consideration. In these cases, the new tradition, if successful, requires the abandonment of its predecessors. In the eighteenth and nineteenth centuries, geological followers of Hutton were hammering out a new tradition, which drew on elements of Caloricist heat theories and Vuleanist geology. In course of time, these research traditions could not be preserved in their pristine form. In view of the same, Huttonians had to hammer out the evolutionary research tradition. These traditions combined previously incompatible elements, quite successfully (Ibid, p.104-5).

Laudan points out that, broadly speaking, philosophers of science agree that cognitive progress is possible if we acquire knowledge by recourse to purely cumulative theories. Cumulative theory means a theory which may add to the store of solved problems, but which never fails to all the problems successfully solved by its predecessors. Put somewhat differently, a necessary condition for one theory, T2, to represent progress over another, T1, is that T2 must solve all the solved problems of T1 and some excess content. So a series of theories is progressive if each later member in the series must entail all the corroborated content of its predecessors and some excess content.

However, such a problem of scientific progress is hardly corroborated by the actual onward march of history. Prior to Hutton, Cuvier and Lyell, geological theorists had been concerned with a very wide range of empirical problems, such as how deposits get consolidated into rocks; how the earth originated from celestial matter and slowly acquired its present form; when and where the various animals and plants originated; how the earth retains its heat; the subterraneous origins of volcanoes and hot springs and the origin and constitution of igneous rocks. Solutions of varying levels of adequacy had been offered in the eighteenth century to each of these problems. However, after 1830, particularly with the emergence of stratigraphy, there were no serious geological theories, which addressed themselves to many of the problems mentioned above. Laudan argues that does it mean that geology was not progressive between 1830 and about 1900? Whereas geological theories after Cuvier and Lyell successfully addressed themselves to a very different set of empirical problems, including those of bio-graphy, stratigraphy, climate, erosion, and long-sea distribution. The same is illustrated within physics by the failure of Newton's optics to solve the problem of refraction in Icelan spar which had been explained by Huygens optics; and by the failure of early nineteenth century caloric theories of heat to explain phenomena of heat convection and generation, which had been solved by Count Rumford in the 1790's. Within chemistry, many problems had been solved by the early theories of elective affinities that were not solved by Dalton's later atomic chemistry. A still better example is offered by Franklinic electricity theory. Prior to Franklin, one of the central solved problems of electricity was the mutual repulsion of negatively charged electrical bodies. Various theories, especially vorticular ones, had solved this problem by the 1740's. Franklin's own theory, which was widely accepted from the middle to the end of the eighteenth century, never adequately came to grips with this problem (Ibid, p.150).

In view of the same, Laudan christens' progress as an aim-theoretic concept. To say, that 'X represents progress' is an elliptical way of saying 'X represents progress towards goal Y'. If our concern is with problem-solving, we can meaningfully speak of progress without reference to accumulation. Assume, for instance, that T1 has already solved problems a, b, and c, while T2 solved a, b, d, f, and g. If our cognitive aim is to possess solutions to the largest number of problems, then clearly T2 is progressive in comparison to T1 (Laudan, 1996, p.119). In problem-solving model of progress we assess the number and the weight of the empirical problems a theory is expected to solve. Similarly we assess the number and weight of its empirical anomalies. Ultimately, we measure the number and centrality of its conceptual difficulties. Constructing the appropriate scales, our principle of progress tells us to prefer that theory which comes closest to solving the largest number of important empirical problems while generating the smallest number of significant anomalies and conceptual problems (Hacking, 1981, p.149). Scientific progress does not necessarily entail that we fully satisfy the condition of accumulation.

Thus, for Laudan, the fundamental scientific products are research traditions. According to Larry Laudan, there are two types of theories. One of them denotes a specific set of related doctrines which can be utilized for making specific experimental predictions and for giving detailed explanation of natural phenomena; for instance, theory of electromagnetism. By contrast the term 'theory' also denotes much more general, much less easily testable, set of doctrines or assumptions; for instance theory of evolution or Kinetic theory of gases. In this case we are referring not to a single theory, but to a whole spectrum of individual theories. The term 'evolutionary theory' for instance does not refer to a single theory, but to an entire family of doctrines that are historically and conceptually related. This kind of theory is the primary tool for understanding. Larry Laudan calls this type of theory scientific research tradition. Problem selections, heuristics, standard of appraisal etc., all are determined by research tradition. According to Laudan, scientific research starts from encountering of a problem and ends with the solution of that problem. So, scientific activity is problem-solving activity. It is rational to accept a research tradition which has greater problem-solving ability in place of research tradition which has less problem-solving ability; problem-solving effectiveness is the standard of judgement in theory choice. Therefore, for Laudan, scientific change is rational and progressive.

IV. Scientific Problems:

In Laudan's model of scientific progress, the solution of the problem is defined as progress. For Laudan, science is essentially a problem-solving activity. Science is an enterprise which is problem oriented. If it can be established that scientific research aims at solving the problem, then we will have to give up that truth is the aim of scientific discourse. Such a stand is radically anti-realistic. Laudan's anti-realism is grounded on his emphasis that science is essentially a problem oriented enterprise or a problem-solving activity. Laudan's philosophy of science gives up truth as the aim of science. According to Laudan scientists work on problems of science which problems are not objectively out their but experienced or felt as tensions by the working scientists. Scientific problems demand solutions and scientific theories are constructed to solve these problems. Theories just try to provide adequate solutions to problems. Thus scientific problems may be said to be the questions of science and scientific theories may be said to constitute the answers. Scientific theories try to resolve ambiguities and reduce irregularities into uniformities. They show the intelligibility and predictability of scientific enterprise. It is in this sense that scientific theories constitute solutions to scientific problems. The crucial test for any theory is whether it provides acceptable answers to interesting problems or whether it provides satisfactory solutions to important problems. Scientific theories aim at solving the problem felt by the scientists (Laudan, 1977, p.13).

According to Laudan, scientists feel various types of problems, such as hard-core scientific problems or empirical problems, intra-scientific problems, normative problems and world-view problems. For example, when we observe that heavy bodies fall towards the earth with regularity and ask how and why they so fall, we pose an empirical problem. Similarly, when we ask an explanation as to why Alcohol left uncovered in the glass disappears, we pose an empirical problem. When we ask why the offspring's of plants and animals bare striking resemblance to their parents, we raise an empirical problem. Anything about the natural world that strikes us as odd or otherwise in need of explanation, Laudan says, constitutes an empirical problem (Ibid, p.14-5). Empirical problems can be unsolved or solved or anomalous problems. Unsolved problems can be resolved in course of time. A solved problem can be counted in favour of a theory. An anomalous problem does not mean a falsifying instance. The hallmark of scientific progress is the transformation of anomalies and unsolved empirical problems into solved ones. As against empirical problems, scientific researchers can also face conceptual problems. Laudan cites the astronomical contributions of Ptolemy. He says the criticism against Ptolemy did not question its adequacy to solve the chief empirical problems of observational astronomy. The criticism was directed against the conceptual credentials of the mechanism Ptolemy used for the solution of astronomical problems. We also experience a conceptual problem when a theory is discovered to be logically inconsistent and self-contradictory Conceptual problems may also arise from ambiguity or circularity.

Intra scientific problems can arise when one scientific theory can go counter to the theory accepted in other parts of science. One of the most powerful arguments put forward against Copernican Astronomy was that it runs counter to the fundamentals of highly established physical theories. Intra scientific conceptual problems emerge because scientific disciplines can never get completely independent of one another. It is so because problems within the domains of physics, chemistry, biology, astronomy etc always criss-cross and interpenetrate. Normative problems can also arise in the scientific domain. Science is conducted by rational agents and has to have certain aims and goals.

There are crucial problems with regard to the articulation and appropriation of those goals. It is through an appropriate methodological strategy that we can achieve some clarity as to what or what not should be done by the scientists with a view to achieving the cognitive, epistemic and practical goals of the scientific research. Historically speaking, such normative issues have been responsible for great debates and controversies with regard to theory acceptance. As science is goal-oriented activity and goals and methods are settled by scientific minds, human goals finally operate within scientific domains. In view of the same, great methodological controversies have cropped up in the history of science on grounds of both method and purpose. Science according to Laudan also experiences what may be called world-view problems. These problems arise when a particular scientific theory is seen to be incompatible with some other body of accepted beliefs. As it happens, our religious and cultural beliefs and values go beyond the scientific domain. For example, the mechanistic theories of physics were discarded by various philosophers of sixteenth and seventeenth centuries for such theories were incompatible with teleological and theological explanations and interpretations inspired by Christianity during those times.

However, for Laudan, the central task of scientific research is to solve problems. In fact, all our scientific achievements are solutions of problems. The essential role of science is to find out solutions to the problems, be it empirical or conceptual. Science can have various aims and objectives and individual scientists can have various motivations for carrying out scientific research.

However, for all practical purposes, Laudan underlines, all scientific research culminates into solutions to ongoing and emerging problems of science. The solution of a problem, however, according to Laudan can never claim to be an attainment of truth. It is true that from beginning of scientific research, to our own times, achievements of science have been nothing but solutions of the problems. These solutions are considered as achievements of science. Scientific progress is defined to be maximising the solutions of the problems and minimising the amount of anomalies and unsolved problems. So, scientific progress means progress in solving the problems. However, the solutions of problems do not amount to achieving the truth. They do not represent the real picture of the world. Scientific problems and solutions are not beyond space and time. They are temporal and spatial problems and solutions. With changing times, the problems as well as their solutions register a drastic reappraisal and reformulation. Perfect adequate solutions to scientific problems of today are deemed utterly inadequate solutions by tomorrow. What counts as a solution to a problem at one time may not necessarily be regarded as such at all times.

Laudan underlines that a research tradition may be highly successful in solving problems. However, it does not mean that the research tradition is confirmed as true. Problem-solving capacity does not tell us anything about truth or falsity of a tradition. A research tradition may be highly successful at proliferating fruitful theories and yet it may suffer from huge ontological and methodological flaws. It can also happen that a particular research tradition is grounded on sound ontological and methodological positions and yet is incapable of generating theories that can solve urgent problems of contemporary scientific research. Rejection of research tradition signifies only a tentative decision to go in for an available alternative that can prove to be more successful at solving problems (Ibid, pp.82-3). As it happens, in course of historical evolution of science, the success of a theory is highly suspect if the theory is linked to an unsuccessful research tradition. Conversely, even an otherwise inadequate theory, will score high on social acceptability if it is associated with a highly successful research tradition. What it amounts according to Laudan is that success of a tradition cannot be regarded as the basis of its truth, and so, failure of a research tradition does not necessarily constitute its falsity. In view of the same, Laudan underlines, a research tradition should not be judged in terms of truth or falsity. Research traditions are products of history. They are created for solving the problems. They are articulated in a particular intellectual context and like other historical products; they register both rise and fall in course of time. Like other historical products, they originate, develop, achieve success or face failures and finally die or peterout and cease to be regarded as serous instruments for scientific progress (Ibid, p.96). A scientific theory can never negotiate truth as a recognisable goal. As rational agents, scientific researchers should have a publically recognisable goal. If the goal is achieved, it should merit public celebration or acknowledgment. Therefore, truth finding should be discarded as a goal of science as a scientific theory can never culminate into a recognisable truth-attainment event. So truth can never be a goal of science or the goal of an individual research (Leplin,j, scientific realism p.196). Historical accounts of science tend to ascribe transcendental properties such as truth or certainty as a goal of science. If science is truth oriented, we can never know which scientific theory is true or approximately true and which is not. We can never bring out to characterise a theory as truth-like nor do we have an epistemic criterion on the basis of which we could legitimately judge one theory to be truer than the other (Hacking I, op.cit, 1981, p.145).

According to Laudan, scientific research originates in the face of scientific problems. It ends the solution of a problem is accomplished. So, problem-solving effectiveness is the central or crucial criterion with regard to the acceptability or unacceptability of a scientific theory or a research tradition. It is rational to accept a research tradition which has greater problem-solving effectiveness, in comparison to a research tradition which has less problem-solving ability. We have to make a theory choice on the basis of problem-solving effectiveness of a research tradition or a particular scientific theory. So, any genuine scientific change, for Laudan, is rational and progressive. The fundamental aim of scientific research is solving the problems; nothing more, nothing less. A research tradition, according to Laudan, is the primary tool of understanding and determines which problems are significant for our research. The solution of these problems does not entail truth. In the face of problem-solving effectiveness or effectiveness of a theory, the truth or falsity of a theory gets irrelevant. Scientific problems are essentially challenges to our intellectual capacities and capabilities and are not necessarily bothered about the appropriation of the objective truth. A scientist operates within a research tradition and it is within that research tradition that solutions to the problems under consideration can be found and personal satisfaction registered. There are no universal and eternal solutions to scientific problems for there are no universal and eternal problems of science as well. Problems and solutions register fluctuations in course of historical evolution. In course of time, the changes have got to take place and these changes are rationally directed by scientists as far as humanly possible. Truth is impossible of attainment, according to Larry Laudan and cannot be set as a goal to scientific research. However, science is rationally directed problems in terms of a quest for solutions to problems.

CHAPTER – 4

Conclusion and Critical Appraisal

CONCLUSION

I have just worked out a sketch of various accounts of scientific progress advanced by Logical Positivists, Karl Popper, Thomas Kuhn, Lakatos, Feyerabend and Stephen Toulmin. I have mainly concentrated on Larry Laudan's account of scientific progress which is the central theme of this dissertation. Now, before we develop a brief critical overview of Larry Laudan's account of scientific progress, a brief recapitulation of this dissertation would be in order.

The Positivists are perhaps best remembered for their efforts to promote the unity of science. They promoted the idea that there was one language of science, a unity of method that becomes codified as a hypothetico-deductive method. This is often interpreted to imply that there is also a unity of laws that generated a reductionism with physics as the most basic science serving as a foundation for chemistry, biology, and psychology and the other social sciences. Correlative with this reductionist world-view was a commitment to what become known as the covering-law model of scientific explanation. Associated with the covering-law model of explanation is a hierarchical model of explanatory connections that can be labelled the "Layer Cake" model. The basic idea is that there are observational facts; for example, about charge a distributions, chemical reactions, embryonic developments, animal behaviours and so on. At the next level up, there will be empirical generalizations about these phenomena. The third stage will include even more general statements connecting the generalizations at the first level. At some stage, theoretical constructions will be introduced. At some level, the sociological laws and theories will be subsumed under the psychological laws and theories, and they in turn will be subsumed under laws of chemistry and finally under laws of physics. This procedure is conceived as continuing indefinitely. Each rise in level brings greater systematization to the body of established scientific knowledge and constitutes scientific progress.

In the 1930's, Karl Popper developed an alternative to the positivist picture of science. Popper accepted the positivist view of the nature of theories and the importance of hypothetico-deductive method but parted company with them on what he saw as the crucial question of how scientific claims are to be validated. The positivists adopted the view that theories were to be validated by being confirmed by evidence. Measures of confirmation were generally taken to be probability measures of some sort. In a choice between two theories, the rule was: choose the theory that is most probable given the evidence. Popper rejected this approach in favour of emphasizing the falsifiability of scientific claims. In this view, the job of the scientist is to subject hypotheses and conjectures to the severest possible tests and to provisionally accept those that passed the most severe.

For Popper, the philosopher of science is a methodologist whose job is to propose a series of methodological rules that will promote the growth of knowledge, that is, the discovery of general laws. These methodological choices are partially a matter of adopting certain conventions based on value assumptions about the aim and nature of science. For Popper, the appropriate choice is the methodology that maximizes the solution of interesting problems. The result encourages methodologies that involve bold conjectures and severe testing of hypotheses. The severe tests to which hypotheses are to be put is a function of a critical method that promotes rational change. Indeed, Popper identifies 'being rational' with 'being critical'. In this way, the progress of science, insofar as it is produced by a critical evaluation of hypotheses, is a rational endeavour.

Popper's evolutionary model of scientific change consists of conjectures and refutations. Problems give rise to hypotheses that are then subjected to serve tests. If the hypothesis fails to pass those tests, it is rejected and another conjecture takes its place. The cycle of testing and assessing brings new texts and assessments. If the hypothesis passes one severe test, then there will always be others more severe yet. Theories or conjectures that pass such severe tests are said to be corroborated. The degree of corroboration of a theory is a measure of how well it has stood up to severe tests in the past. But it says nothing about the likelihood of the theory notwithstanding future tests. So, in what sense is the most highly corroborated theory the best choice in a given situation? Popper's argument for identifying the most highly corroborated theory as the best goes along line like these: 1). the rational choice is the best choice, 2). It is always rational to choose the option that has been severely tested, and survived, because those theories are the ones that have been subjected to the severest criticism. (For Popper, to be rational is to be critical.)

3). In the case of theories, it is rational, then, to choose that theory among the competing options that is most highly corroborated. Therefore, 4.) The best theory is the most highly corroborated one.

The most highly corroborated theories also "track" the truth, in the sense in which Popper argued, viz; a measure of closeness to the truth that is called "verisimilitude" could be defined. With such a measure, it would be possible in principle to determine which of a set of alternative conjectures represented genuine scientific progress. This proposal generated an extensive literature designed to refine and validate such a measure. For the most part, such attempts to construct viable measures of verisimilitude have come to naught, although there are still defenders of this approach as the best method for judging scientific progress.

The Growth of a scientific discipline, in Kuhn's view, follows a standard pattern of different stages. In his earliest formulation of this stance, it is possible to distinguish five stages that characterize the progress of science. These are: 1. Immature science. 2. Mature (normal) science. 3. Crisis science. 4. Revolutionary science. 5. Resolution; normal science resumed.

The cycle then repeats itself through stages 2-5 indefinitely. Central to Kuhn's view is the notoriously slippery concept of a "paradigm", or "disciplinary matrix". A disciplinary matrix is the overall collection of methods, formulae, rules, procedures, and commitments that govern scientific research. Kuhn came to distinguish four major components of disciplinary matrices: symbolic generalisations, models, values, and exemplars. These are the shared standard examples that give "content" to the abstract principles of the disciplinary matrix. The exemplars are the fundamental units in the matrix and are the basic tools by which the scientist working in a normal science tradition advances the range of phenomena rendered by law to the basic principles and theories of the tradition.

In structure, immature science is characterized as pre-paradigmatic. Later, Kuhn argued that no research occurs in the absence of paradigms. Immature science is the science that is characterized by paradigms, which for some reason or other; fail to generate a "puzzle-solving" tradition. The shift from immature to mature science is then seen as a shift from a paradigm that "leads nowhere" to one that provides a context of unsolved puzzles and problems such that some hope exists for solution.

Mature (normal) science is thus a problem-solving tradition. According to Kuhn, most scientists spend most of their lives working in such traditions. The normal science tradition seeks to extend and entrench tried-and-true theories and practices. Progress at this stage consists in increasing the number of systems that can be understood in terms of the fundamental exemplars of the theory. However no theory or paradigm successfully solves all its problems. Problems that resist assimilation to the techniques of the paradigm are labelled 'anomalies' when the failure of a paradigm to reduce anomalies to lawfulness is perceived (by scientists) to be as greater than the power of the paradigm to force nature into a crisis of results.

Crisis science is characterized by a general recognition that the ruling paradigm is no longer functioning effectively. This recognition may come from the community of scientists working within the tradition of the disabled paradigm. Break-down comes with proliferation of new ideas, theories, methods, and alternative paradigms.

When one of the new paradigms begins to emerge as a contender for succession, the result is a conflict between the new and the old paradigm. Since paradigms are pervasive in determining worldviews, the ground rules for deciding among competing paradigms are not the same as those that operate within a single tradition. Kuhn argues that not only are the worldviews of different paradigmatic traditions different, but, in a sense, the world itself is different for the practitioners in different paradigmatic traditions.

The ultimate resolution of paradigm conflicts results in the emergence of a new normal science tradition. According to Kuhn, such resolutions involve something akin to a gestalt shift. Critics were quick to conclude that, for Kuhn, scientific progress from one paradigm to another is fundamentally an irrational process, since the standards and values of one paradigm to another is a fundamentally irrational process. In responding to his critics, Kuhn sought to soften this implication by pointing out that there were certain values characteristic of science, such as simplicity, predictive accuracy, and requirement for consistency, that transcended particular disciplinary matrices. Thus, he argued, revolutionary science was both progressive and rational.

Alongside Kuhn was Imre Lakatos, who developed a blend of Kuhnian and Popperian elements that he labelled the 'methodology of scientific research programs'. He argued that the fundamental unit of assessment should be a research tradition such as Newtonian mechanics, rather than a single hypothesis or conjecture. These research programs consisted of a "hard core" of central symbolic generalizations, along with a "protective belt" of auxiliary hypotheses. Programs were progressive, when they focused on protecting the core from refutation. The resulting model of scientific development was claimed to be a sophisticated falsificationist version of Popper's program that offered a response to what Lakatos saw as Kuhn's irrationalist view of scientific change.

Feyerabend is another important philosopher of science. On the matter of falsifiability, Feyerabend argues that no theory is always consistent with all the relevant facts. Like Lakatos, he sees the use of ad-hoc postulates to save the leading paradigm as an essential to the progress of science. However, Feyerabend goes much further than Lakatos; taking example from the history of science, he claims that scientists often depart entirely from the scientific method when they use ad-hoc ideas to explain observations that are only later justified by theory. To Feyerabend ad-hoc hypotheses play an essential role; they provisionally make a new theory compatible with facts until the theory to be defended can be supported by the other theories.

On the matter of paradigm-shift, Feyerabend emphasises Kuhn's idea that the reigning paradigm deeply influences interpretation of observed phenomena. However, he adds to this by suggesting that in the paradigm model the reigning paradigm would also have a shifting influence on the incoming theory; instead of being dictated by agreement with observation alone, the new theory must also concur with the old one. Putting the two points together, Feyerabend concludes that it is impossible to view the progress of science in terms of one set of methodological rules that is always used by scientists: such a 'scientific method' would in fact limit the activities of scientist and lock up scientific progress. According to universal and fixed rules, Feyerabend suggests that science often progresses by ad-hoc guesses that break the rules by the attitude of "anything goes". This "anything goes" is formally known as epistemological anarchism.

Feyerabend on the other hand rejected the idea that scientists are rational creatures entirely. In his book "Against Method" he argues that scientists do not, in fact, follow any one given method but are much less rational than their propaganda makes them out to be.

Toulmin has laid emphasis on the question of the rationality of scientific discovery. He has provided an anti-positivistic account of rationality of science. He has brought out that scientific arguments and the arguments advanced in other disciplines expect the pure sciences are imperious to description or explanation in terms of formal logic. Neither deductive nor inductive methods of inference can capture the enterprise of scientific research. It is rather the method of representation – a sort of graphical, pictorial image of given physical phenomena. That plays a pivotal role in our understanding of science.

As a philosopher of science, Toulmin brings out the limits of methodology. He is convinced that formal methods cannot be helpful in trying to understand the nature of scientific practices. A scientist has to go beyond methodology and formal logic in order to carry out scientific research. A scientific discovery is not a function of methodological instructions or formal algorithms. It is rather through professionally trained imagination that we can negotiate any real scientific breakthrough. The scientific reasoning can fruitfully be compared with juristic reasoning. Like legal regulations, scientific laws too are not universal by definition. They need to be suspended or changed in the light of given circumstances. A scientific situation resembles a legal situation for both of them negotiate new or complex disputes which cannot be successfully explained by pervious rules and methods. Sometimes both the scientists and the jurist have to go beyond the established procedures and creatively workout a new scientific paradigm or a new legal paradigm. In view of the same, Toulmin suggests that, we need always to challenge the rules of rationality and deem the results of our research as tentative and revocable.

Rationality is the characterizing feature of Laudan's model of science. However, even more fundamental than the notion of rationality is the notion of progress in his account of science. He does not define progress in terms of rationality; he rather defines rationality in terms of progress. "In a phrase, my proposal will be that rationality consists in making the most progressive theory choices, not that progress consists in accepting successively the most rationality theories."

Laudan's account of science has two characterising features. Firstly, any adequate model of science must be able to accommodate scientific change. Secondly, any adequate account of science must recognize that scientific rationality and progress are not linked to the truth, verisimilitude, corroboration, falsifiability etc of scientific theories but with their problem-solving effectiveness. Laudan brings out that scientific evolution entails that we make a distinction between mini and maxi theories. A mini theory is a 'very specific set of related doctrines.... which can be utilized for making specific experimental predictions and for giving detailed explanations of natural phenomena'. A mini theory cannot negotiate a change in view of the fact that any slight change in such a theory can provide us the sufficient warrant to stipulate it as a new theory.

On the other hand, a maxi theory, according to Laudan, is a research tradition. It is comprised of a number of ongoing and proceeding theories and sets of doctrines and assumptions. It has a set of metaphysical and methodological commitments as well. Thus a maxi theory or a research tradition is much more general and much less testable. A research tradition is often characterized by powerful normative elements. In view of the same, it is weak on explanatory power, productivity and testability. All research traditions go through different and often mutually contradictory formulations. A research tradition negotiates evolution by recourse to successive replacement of its constituent theories. At times, even some of its basic or core elements are imperceptibly given up and replaced by another set of basic elements. All research traditions, at any given point of time, are characterised by certain metaphysical and methodological commitments. However, even such predilections can gradually change or shade off.

According to Laudan, problem-solving is the fundamental aim of all intellectual endeavours. Such solutions shape up as theories in scientific research. Sometimes day-to-day problems can become genuine scientific problems. Sometimes common sense solutions can graduate to genuine scientific theories. Broadly speaking, Laudan divides problems into two categories – empirical and conceptual problems. An empirical problem is 'anything about the natural world which strikes us as odd and in need of explanation.' It is true that, a genuine empirical problem has to be about the world. However, in order to be deemed or defined as an empirical problem, it need not necessarily be accurately describing a real state of affairs. The minimum requirement is that a competent scientific researcher must think it to be describing an actual state of affairs. Furthermore, a problem-solving theory need not be true. No theory so far has been certified to be demonstrably true. The theories too need not be certified to be fully free of anomalies with view to functioning as solutions to the problems. Nevertheless, only in the context of a theory can a problem be deemed or defined to be a problem and there is only one way of solving a problem, viz; by deriving an approximate statement of the problem from a particular theory.

Laudan classifies empirical problems into three kinds; (1) Solved problems (2) Unsolved problems (3) Anomalous problems.

1. Unsolved problems: Those empirical problems which have not yet been adequately solved by any theory.
2. Solved problems: Those empirical problems which have been adequately solved by a theory.
3. Anomalous problems: Those empirical problems which a particular theory has not solved, but which one or more of its competitors have.

To begin with, theory related problems pose a difficulty with regard to an 'unsolved problem'. If we start with assumption that the state of affairs can be deemed problematic only in relation to some theory, then any account of a state of affairs which is neither derivable nor compatible with any theory can be deemed to be only a curiosity. We cannot deem it to be a problem. Laudan's solution to such difficulties is to demote unsolved problems to the status of 'potential problems' pending their solutions.

Laudan concedes that meaning may not be theory-independent. It is possible that language embedding our conceptual networks may furnish an inextricable 'tint' to what we perceive. Nevertheless, Laudan is not necessarily bothered by the so-called problems posed by the thesis of 'incommensurability'. It is not necessary that any of two theories have got to conflict because all terms are theory-laden. No state of affairs can operate as the same problem for two different theories. In view of the same, no anomalous problems need necessarily arise in this regard. Laudan argues that it is wrong to assume that two theories can be compared only if theoretical assumptions of the problem under consideration are different from theories attempting their solutions. Besides, theories need not necessarily be compared on the basis of commensurability. There are other parameters for comparison such as internal consistency, simplicity, precision of prediction etc.

Laudan also underlines the significance of conceptual problems in scientific research. As a matter of fact, conceptual problems are more serious for a scientific theory than the empirical problems. It so happens that empirical problems are not that intimately linked to theories as are conceptual problems. In comparison to conceptual problems, empirical problems enjoy a measure of autonomy. The conceptual problems have no independent existence; they are existentially linked to theoretical constructions. Laudan subdivides conceptual problems into two; firstly, conceptual problems such as inconsistency, vagueness and uncertainty can arise within a particular system. Secondly, conceptual problems may arise from a disagreement between two rationally justifiable theories. There are various elements in the world views, research traditions and even at the level of specific theories which do conflict with one another. In the same way, Darwinian biology was quite compatible with the scientific developments during Victorian era. However, Darwinian biology was radically incompatible with highly entrenched metaphysical and teleological theories such as essentialism. Laudan concedes that such anomalies and discrepancies do happen in the ongoing evolution of scientific investigations. Laudan is happy even to extend this line of reasoning to principles of theology, metaphysics and methodology – to the extent such principles have been instrumental in successfully solving scientific problems in the past. Laudan's notion of rationality is much broader than the rationality espoused by other philosophers of science. Laudan says that even non-empirical and non-scientific factors can legitimately contribute in the rational development of science. It can be argued that nineteenth century scientists where perfectly rational in accepting Darwinian biology as it rested on scientific causality or naturalistic assumptions or it indicated the perfectible nature of human evolution. We can argue with equal plausibility and rationality that Darwinian biology was acceptable as it was not sufficiently inductive. We can also take the stand that Darwin's theory was not acceptable because it was not in accord with the wisdom and beneficence of God or it constituted a virtual degradation of man.

Laudan is not in agreement with Feyerabend's stand, viz; 'anything goes'. To begin with, he draws our attention towards non-cognitive elements in our taken sides in the domain of science. For example, those scientists who accepted Darwin's biology with a view to ingratiating themselves with the Darwinian power elite or rejected it for fear of subverting democracy do not have a shred of scientific or rational credibility. Moreover, we cannot say that all cognitive influences do have an equal good impact. For instance, before the arrival of Darwinian paradigm, natural theology was on the way to decline.

Thus Laudan sees science as a second-order activity; it has only descriptive force and no rational or normative force. He push's aside the truth question in favour of problem-solving effectiveness. He understands truth as correspondence and correspondence is unrealizable. Explanations are valid and acceptable until falsified. If truth, not rationality, were science's aim, then that aim would be utopian and science could be shown to be neither progressive in the comparative progress of its solutions, nor judged in terms of our aims and values. But it does not for that reason, discover truth.

But truth need not be understood as correspondence. Another option is possible in which doctrines can be envisaged as being simultaneously referential. Reference need not be predicated on an equivalence of meaning between discourse and reality. The reference of doctrines, as second-order language, is not an ostensive reference pointing to a state of affairs but is a prospective or heuristic reference. They do not describe the world as it is but as it could be. Doctrines project practical possibilities of life and of being in the world. The truth of doctrines can be defined in terms of Laudan's concept of realizability and not correspondence. Such truths can be measured, for example, in well-designed outcome and longitudinal studies in terms of success. Success is judged, as Laudan argued, in accord with goals and values.

The upshot of Laudan's philosophy of science is that the fundamental objective of scientific research is trying to explore or discover solution of cognitive problems which are basically of two types – empirical and conceptual problems. Scientific theories are not to be apprised in terms of truth or falsehood but in relation to their problem-solving effectiveness. A theory having greater problem-solving effectiveness is more apt and relevant or better than a theory with lesser problem-solving effectiveness. It is the problem-solving effectiveness of scientific theories that characteristically defines scientific progress. Maximisation of scientific progress constitutes the essence of scientific rationality. However, theories go on changing and scientific rationality consists in using the best available specific theories. It is a research tradition which suggests and sets problems, evaluates their significance and suggests ways of solving them. In view of the same, an evaluation of a scientific progress or scientific rationality entails comparison of research tradition. Whether any given research tradition is adequate or inadequate is itself a function of the problem-solving effectiveness of those scientific theories which together constitute a research tradition.

CRITICISM

Philosophers of science have advanced highly accomplished theories or accounts of scientific progress. Every theory or account can have its merits and demerits or strengthens and weaknesses. However, what is fundamentally important is that they advance highly conflicting views with regard to scientific progress. There is wide spread disagreement among philosophers of science in this regard. They have not been able to provide a consensual and coherent account of scientific progress. They disagree as much with regard to scientific progress as classical metaphysicians were disagreeing with regard to nature of reality or modern European philosophers were disagreeing with regard to origin and nature of knowledge. Now, whom we are to accept or whom we are to reject, is a highly critical question. We have six accomplished accounts of scientific progress. They are presenting radically different theories with regard to scientific progress. There can be other philosophers of science who can also advance various other types of theories with regard to scientific progress. In such a situation it gets exceedingly difficult to accept any account as true and rational and any other account as false and irrational.

Like most philosophers, Laudan believes that by and large science makes cognitive progress and that the development of science is more or less rational.

Problem-solving activity is central to scientific research, according to Larry Laudan. Most of scientific problems are generated while pursuing prior problems. Accordingly, sound judgments regarding success and progress in science require delimitation of the appropriate field of problems and comparison of the total problem-solving effectiveness of the various relevant theories. Such deeply historical and comparative judgments are not adequately analyzed by any traditional account of scientific rationality. Laudan considers it rational to pursue a theory when its relative rate of progress (of solving or of promising to solve scientific problems) is high, and when its overall success as a problem solver is also high in comparison with those of its competitors.

According to Laudan, problem-solving effectiveness is the measure of progress in science. Accordingly, he must provide adequate means of identifying and individuating scientific problems. He does not attack this task face-to-face, nor does he succeed in it. Nonetheless, he contributes usefully to it and to the problem of providing criteria of adequacy for the solution of scientific problems. He divides these problems into two principle classes: empirical and conceptual. At any given time serious theories has solved certain empirical problems and fail to solve others. Among the later, those which have been solved by competitors count as anomalies. They do not falsify or force rejection of the theory, but they do cast significant doubt on its problem-solving effectiveness. The conceptual problems a theory faces are either internal or external. The latter include conflicts with neighbouring theories (e.g., conflict between Copernican astronomy and pre-Galilean mechanics) and conflict with accepted methodological norms.

We can measure the overall problem-solving effectiveness of a theory by assessing the number and importance of the empirical problems which the theory solves. It is from such effectiveness that we can deduct the number and importance of the anomalies and conceptual problems which the theory generates. Laudan explores and examines problems in theory evaluation. Although he does not produce the utopian calculus he requires namely one in which each unsolved anomaly and conceptual problems count so much against it, he identifies various factors which increase the evaluative importance of a problem (e.g., acquiring the status of an anomaly, serving as a model for a large class of problems) and others which decrease its evaluative weight like being shifted to the domain of another theory. Besides, he offers a way around the Quine-Duhem problem of fixing the blame for predictive failure and the credit for problem solutions. Since his concern is not truth but problem-solving effectiveness, the entire theory complex employed in dealing with a problem shares equally in the blame for failure or the credit for success.

For Larry Laudan, a scientific theory covers a range of phenomena falling under two ideal types. Firstly, there are determinate set of specific, interrelated axiomatizable doctrine (e.g., Maxwell's theory of electromagnetism). Secondly, a theory covers a set of general assumptions about the entities and processes in a domain of study, and about the appropriate methods to be used for investigating the problems and constructing the theories in that domain. The relations between theories and research traditions are a major source of conceptual problems: the methodology or ontology of a research tradition may fit at well with a particular theory when a scientist is forced to develop an account for certain phenomena. Nevertheless, research traditions are not dispensable; they guide the development of theories and help rationalize and justify the pursuit and acceptance of particular theories. Success and rapid progress in a research tradition justify the search for a theory along the lines prescribed by its ontology and methodology and contribute to the positive appraisal of theories well integrated into that tradition. Since research traditions gradually evolve and are able to take over theories formerly belonging to unknown traditions, evaluation of their success, like that of narrower theories, is a thoroughly historical process. Evaluation of progress, and hence rationality, in science requires evaluation of the problem-solving effectiveness of both kinds of theories.

Laudan has often underlined that evaluation of scientific theories is comparative. If so, his argument in favour of his own theory of scientific progress is radically incomplete, for inspite of specific criticism of Kuhn, Lakatos and others, he does not set forth their theories carefully enough to allow the reader to estimate the full force of the specific criticisms he directs against his competitors.

Laudan often stresses that his system of theory assessment is workable. In fact he deems it to be one of its major virtues. But, in the absence of a rigorous calculus, he does not show convincingly that informal assessments of problem-solving effectiveness and rates of progress need yield consensus except, perhaps long after the fact, when the relative importance of problems has been settled in the light of subsequent theories and empirical results. It is therefore, not clear whether his methodology is any more workable than Lakatos's, in which one can judge the importance of problems and the adequacy of solutions only with hindsight and cannot discount any idea, no matter how wild, on the spot.

Laudan while rejecting traditional philosophical attempts, at solving anomalies encountered in various theoretical formulations, does not offer any indefeasible solution to the problem under-consideration. He is a hardcore epistemological pessimist. He does not define the aims and objectives of scientific research in terms of truth. He does so because according to him we can never recognise true theories. We cannot even determine which of the theories sound to be increasingly approximating to the truth. Instead, Laudan tries something more practical, something that could be used in real scientific cases. Instead of trying to measure up the rationality and truth of scientific discourse, Laudan, offers us the criterion of problem-solving effectiveness. Now, it can be plausibly argued that it is as difficult to measure up problem-solving effectiveness as it is to measure up rationality and truth of scientific theories.

Laudan does not have the courage to face the philosophical difficulties encountered by other philosophers of science. He is avoiding the issues with regard to confirmation, corroboration, content, verisimilitude etc. He avoids the puzzles about incommensurability as well. Laudan tries to simply difficulties by defining progress in simpler, clearer and easier concepts. However, Laudan does not demonstrate that his quest for simplicity and clarity leads to any solutions of various difficulties encountered in the long drawn-out scientific research. Laudan's 'research tradition' account while focusing on problems does not provide us a better understanding of the dynamics of science. He fails to bring out the motivations which inspire scientific investigations.

It is not clear as to what a scientific problem is, according to Laudan. What counts a solution or a satisfactory solution of problems under consideration? Laudan thinks that scientific problems arise when someone thinks that a particular claim about the world is true and deserves an explanation. This sounds to be a subjective or sociological account of scientific problems. Even if we accept Laudan's psychological and sociological dimensions of scientific problems, Laudan will still have to explain as to why some facts are felt to be in need of explanation whereas, other facts do not press for an explanation. He will have to explain which facts should be taken seriously and which should be consigned to sidelines of scientific research. Else his account of scientific problems will be highly vulnerable to individual idiosyncrasies. Laudan will have to provide a really tight theory of what actually counts as an acceptable solution to any problem under consideration.

Laudan emphasises that, "the criteria for what counts as solving a problem have evolved so much that what was once regarded as an adequate solution ceases to be regarded as such" P.25 (1977). It can be conceded that the substance of solutions changes in course of time, especially as our frontiers of knowledge expand. With increase in knowledge problems change, what count as solutions change, the standards of the precision and accuracy of the experimental measurements change. However, Laudan cannot substantiate that logical and epistemological criteria for determining the problem and their solutions go on changing in the field of scientific research. Laudan says that a solution of a scientific problem is that which confirms to the standards operating at the time. However, Laudan does not tabulate those very standards for, according to him, they go on changing as well. Thus, Laudan effectively fails to establish as to what counts as problem in science and what counts its solution. Laudan cannot present a theory of scientific progress unless he presents an account of scientific standards. Laudan is not providing such an account.

We may accept Laudan's claim that evaluation of theories needs comparative analysis. However, he does not subject his own theory of scientific progress to comparative analysis. He does not compare his theory of problem-solving effectiveness with the theories of Kuhn and Lakatos, in spite of their specific criticisms of Laudan's interpretation of science. Laudan's own criticism of Kuhn, Lakatos and others is also not clearly brought out for he does not delimit the field of his problems. He does not offer any convincing argument that could establish the superiority of his own theory in terms of problem solving effectiveness and also specify the relative weakness of the theories advanced by his competitors. We cannot know the workability of

Laudan's system. We cannot be sure whether his methodology is comparatively workable or not.

Laudan radical espousal of instrumentalism is neither prudent nor judicious. He advocates that rationality consists in maximising the number of solved empirical problems and minimizing the number of unsolved conceptual problems. Such an analysis, according to Laudan, needs not to be integrated to an analysis of rationality in terms of seeking approach to the truth. Laudan's extreme instrumentalism makes it impossible to delimit conceptual problems as well. On Laudan's analysis, there is no wrong in employing conflicting modes of description with refrence to various phenomena in various contexts. It is so because truth is not an issue at all. We cannot be sure whether Laudan's instrumentalist approach does help or hinder the development of his theory of progress.

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